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THE
LONDON, EDINBURGH, AND DUBLIN
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

CONDUCTED BY
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AND
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"Nec araneorum sane textus ideo melior quia ex se fila gignunt, nec noster
vilior quia ex alienis libamus ut apes." JUST. LIPS. *Polit. lib. i. cap. 1. Not.*

VOL. XLI.—FOURTH SERIES.

JANUARY—JUNE 1871.

LONDON.

TAYLOR AND FRANCIS, RED LION COURT, FLEET STREET,
Printers and Publishers to the University of London;

SOLD BY LONGMANS, GREEN, READER, AND DYER; SIMPKIN, MARSHALL AND CO.;
WHITTAKER AND CO.; AND KENT AND CO., LONDON:—BY ADAM AND
CHARLES BLACK, AND THOMAS CLARK, EDINBURGH;
SMITH AND SON, GLASGOW; HODGES AND
SMITH, DUBLIN; AND PUTNAM,
NEW YORK.

"Meditationis est perscrutari occulta; contemplationis est admirari
perspicua Admiratio generat quæstionem, quæstio investigationem,
investigatio inventionem."—*Hugo de S. Victore.*

—"Cur spirent venti, cur terra dehiscat,
Cur mare turgescat, pelago cur tantus amaror,
Cur caput obscura Phœbus ferrugine condât,
Quid toties diros cogat flagrare cometas;
Quid pariat nubes, veniant cur fulmina cœlo,
Quo micet igne Iris, superos quis conciat orbes
Tam vario motu."

J. B. Pinelli ad Mazonium.

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CORRIGENDA IN VOL. 40.

Page 437, line 6, for A_{m-1} read A_{m+1} .

— 440, omit the paragraphs on this page of Mr. Moon's paper.

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AND
JOURNAL OF SCIENCE.

[FOURTH SERIES.]

JANUARY 1871.

I. *On the Examination of the Bessemer Flame with Coloured Glasses and with the Spectroscope.* By J. M. SILLIMAN, M.E.,
*Adj. Prof. of Metallurgy, Lafayette College, Easton, Pa.**

I. *Examination with Coloured Glasses.*

IN the Bessemer process the progress of the decarbonization is determined chiefly by the appearance of the smoke, flame, and sparks which are emitted from the apparatus. Owing to the rapidity with which the reactions take place, it is highly important to catch the exact moment when the blast should be turned off. This is indicated by the colour and brightness of the stream of gas issuing from the converter; and by this the moment of total decarbonization can generally be accurately determined by the naked eye. When, however, pig iron of certain qualities is used (manganiferous iron, for example) this determination is very difficult; even those who have had much experience make frequent mistakes, and find it impossible to produce the same quality of steel at every blow.

In order to intensify these flame-indications, use has been made of the spectroscope, and also of various combinations of coloured glasses. The former was first attempted by Dr. Roscoe, and the latter by Mr. Rowan at the Atlas Works.

Mr. Rowan experimented with a great variety of coloured glasses, and obtained the best results by using three glasses—two of ultramarine-blue and one of dark yellow. This little instrument (or chromopyrometer, as he terms it) is now in daily use at the Atlas Works, its indications being so marked and un-

* From Silliman's American Journal for November 1870.

mistakable as to render its use safe in the most inexperienced hands.

The following experiments were made at the Bessemer Steel Works of John A. Griswold and Co., in Troy, while pursuing the chemical course in the Winslow Laboratory of the Rensselaer Polytechnic Institute. In my observations on the flame, I made use of the spectroscope and also of a combination of coloured glasses. This combination consisted of two light-yellow glasses and a blue one, through which the sunlight appeared of a deep purplish-blue tint; and as it differed slightly from Rowan's, it gave somewhat different results.

In order to reproduce the appearance of the flame at the different stages of the process, I prepared a plate consisting of about a hundred varieties of colours and tints, all of which were numbered, and thus referred to a Table which indicated their composition. They were also arranged to be seen with either a light or dark background. The use of this plate was of necessity limited to daylight; but the illustration and description are given as occurring at night in order to show its illuminating-power.

At the beginning of the process that which issues from the converter does not appear to be a true flame, but only an illuminated stream of gas carrying with it innumerable red-hot pellets of iron. This gas has scarcely any illuminating-power, extends but a short distance from the mouth of the converter, and is sometimes sheathed with a whitish smoke. Seen through the glasses, the flame and sparks have a deep crimson colour, the converter is invisible, and at the base of the flame is a crimson band which continues throughout the process.

As the reaction continues, this stream of gas grows brighter and more elongated, and after a few minutes a small pointed whitish flame appears which suddenly increases in size. At this instant the blast-pressure falls from twenty to eighteen pounds.

When viewed through the glasses, the upper part of the converter comes dimly into view, and the flame and pellets of iron appear of a lighter colour, while the fragments of slag which begin to be thrown out are of a deep red. This difference in shade between the iron and slag thrown out is probably entirely owing to the lower temperature of the latter—for the reason that, while the iron is discharged from the metallic bath, the slag is washed up on the sides of the converter, and can be seen clinging around its mouth in a spongy mass until detached and thrown out by the blast. The greater porosity of the slag and its consequent more rapid cooling would also cause a difference of temperature.

In the second period the discharge of slag increases, and the flame is very bright and illuminating with occasional dark streaks. Through the glasses at the beginning of this period the flame is of an ashy-blue colour with streaks and flashes of crimson, the edges being sometimes of a purplish hue. At this point surrounding objects are illuminated, and the converter becomes distinctly visible. A wreath of crimson is seen surrounding the flame where it strikes the chimney. By the middle of this period the crimson almost entirely disappears from the body of the flame, leaving only a slight cone at its base, and a border of greenish hue makes its appearance and gradually grows more decided. Streaks of a dark blue colour are also seen in the body of the flame.

The beginning of the third period is scarcely indicated to the naked eye, though the flame becomes somewhat weakened, and after a few minutes shows dark streaks running through it. Through the glasses, at the commencement of this period the rose-coloured cone begins to expand and deepen, the greenish sheath is more decided, while streaks of dark and green are visible. After a few minutes the change becomes very rapid, a few seconds only being required to reduce the flame from rose-colour to the deep crimson non-illuminating gas as at first, and again the converter is lost to view, by which time the blast should have been turned off.

The gradual fading of the crimson from the beginning of the blow and its deepening at the termination of the process, as well as the crimson band at the base of the flame and the wreath of crimson surrounding the flame at the chimney, tend to confirm Mr. Rowan's views, which are that the different shades of crimson are due to changes of temperature. The stream of gas which comes from the mouth of the converter at the beginning of the process being illumined from within, derives its colour from the metallic bath, the temperature of which, owing to the combustion of silicon, increases more rapidly during this period than at any other.

The crimson band at the base of the flame and the wreath of crimson at the chimney might also be accounted for by this theory. The flame rushing from the mouth of the converter has a tendency to create a vacuum at its base around the converter's edge, and thus to cause a wreath of flame to pass over this surface, and by consequent cooling produce the crimson band. The wreath of crimson at the chimney may be also due to the cooling of the flame consequent upon deflection.

It is true we have a seeming contradiction to this theory in the rose-coloured cone extending from the base at the centre, which we should naturally consider the hottest part of the flame ;

but, as in the flame of the Bunsen burner, the hottest part is in its outer sheath; the conditions of combustion in both being similar, it is probable that that part of the flame occupied by the cone is at a lower temperature than that surrounding it.

The green streaks in the flame are most intense when the manganese spectrum is brightest; and as the colour of the flame when the Spiegeleisen is added is also green, we are led to suppose them due to the presence of manganese.

On two occasions simultaneous observations were made with the spectroscope and the coloured glasses; but, with the exception of that just mentioned, and the changes at the commencement and termination of the blow, no striking coincidence was noticed.

II. *Examination with the Spectroscope.*

The science of spectrum-analysis is yet in its infancy; and there has been no scientific investigation, perhaps, which has been more contradictory in its results than that of the Bessemer flame. The first application of the spectroscope to the analysis of the Bessemer flame was made in 1862, by Dr. Roscoe, at the works of Messrs. John Brown and Co., in Sheffield. Soon after this it was in constant use in Brown's works for controlling the process. It was next introduced at Crewe, and from there is said to have been taken to Seraing, in Belgium, in 1865.

Roscoe's account of the general appearance of the spectrum has not altogether been verified by subsequent observers. His not having seen any line beyond 80° indicates an imperfection in his instrument. He also is the only one who claims to have seen the sodium-line as an absorption-band, or who professes to have detected the lines of nitrogen and hydrogen in the Bessemer spectrum. His spectroscope was so arranged that the spectrum of the Bessemer flame was seen in the upper half of the field of view, while the spectrum with which it was to be compared was seen immediately below. The spectrum of the flame was thus compared with the following spectra:—

1. Spectrum of electric discharge in carbonic-oxide vacuum.
2. " strong spark between silver poles in air.
3. " " " iron "
4. " " " " hydrogen.
5. Solar spectrum.
6. Carbon spectrum—oxyhydrogen blowpipe supplied with olefiant gas and oxygen.

The coincidences observed were very few, and totally failed to explain the value of the Bessemer spectrum. The lines of the well-known carbon spectrum did not occur at all, either as

bright lines or absorption-bands, nor was any coincidence observed between the lines of the Bessemer spectrum and those of the carbonic-oxide vacuum-tube. The lines of lithium, sodium, and potassium were strongly marked, and identified with certainty. He found that three fine bright lines between E and *b*, shown on the plate at $66\frac{1}{2}^{\circ}$, 67° , and $67\frac{1}{2}^{\circ}$, coincided with those of iron; and in place of the red hydrogen-line C he discovered a black band which he considered an absorption-band, and states that it is better defined in wet than in dry weather.

In Austria Professor Lielegg followed up this subject with great perseverance, and gave more extended accounts of the varying character of the Bessemer spectrum during the different stages of the process. His experiments were made at Gratz, where the spectroscope was afterwards used with great success in controlling the Bessemer process; but at Königshütte, where dark grey manganiferous iron was used, it was found that the indications which in other works so plainly determined the moment of decarbonization were unreliable. In this case the lines, whose disappearance is to indicate the exact point of time for ending the process, disappear too soon. During the period in which the spectrum is brightest, among the glowing vapours and gases that stream from the converter, carbonic oxide, next to nitrogen, is most abundant; and it is for this reason that the first investigator (Roscoe) expressed himself as confident that the numerous lines of the spectrum were caused by this gas, although he could obtain no coincidence.

Brunner* states that "no part of the Bessemer spectrum is ever visible in the flame when the converter is heated for the first time after being relined, but, that when the lining is not new, Lielegg's group of green lines ($\text{CO}\gamma$) appears in the spectrum, which then contains also the lines of potassium, sodium, and lithium,"—from which he concludes that this spectrum is not to be identified with carbonic oxide, but must be produced by other constituents of pig iron. Others state that the Bessemer spectrum is sometimes visible while the converter is being heated after a blow. I made an observation of the flame from the converter while it was being heated the first time after being relined, and obtained with great distinctness the potassium-, lithium, and sodium-lines, but have not under any circumstances detected any other lines while the converter was being reheated.

Lichtenfels, by a series of simultaneous comparisons of the manganese with the Bessemer spectrum, found the lines in the blue and green fields to completely harmonize in the two spectra. The violet manganese-line which had been seen by some he

* Van Nostrand's Eclectic Eng. Mag., vol. i. p. 508.

could not detect in either of the spectra. I have never observed it; but Dr. Wedding, who has summed up the observations of others, states that he has repeatedly seen it. Its position is at $135\frac{1}{2}^{\circ}$.

The instrument used in my investigations was constructed by Alvan Clark of Cambridge, and consists of an equiangular flint-glass prism, in a metallic box, into the sides of which at the requisite angles are screwed an inverting-telescope with a magnifying-power of six, and a tube containing the adjustable slit and lens for rendering the rays parallel—also a tube with a scale, which is placed at such an angle that it is reflected from the surface of the prism through the telescope to the eye; it can be so adjusted as to appear along the upper edge of the spectrum. I was provided with Bunsen's plates of spectra on a large scale; and in order to adapt them to the scale in my instrument, I took the spectrum of the sun and obtained Fraunhofer's lines with great distinctness. Two characteristic lines in the solar spectrum were then noted, one of which appeared at 37° and the other at 117° , and a space measured equal to their distance apart as given on Bunsen's scale. This was divided into eighty equal parts, and the division extended in both directions. By the application of this scale to Bunsen's, I found that the remainder of Fraunhofer's lines in my instrument exactly coincided with their position on his plates. The correctness of the new scale was also proved by other coincidences. By moving the prism, Fraunhofer's lines will vary slightly in their relative distances apart; but in no possible position in which I could place the prism could I obtain the sun-spectrum as given by Wedding in connexion with the Bessemer spectrum; if the spectrum given by him was obtained by the use of bisulphide of carbon in his prism, that substance causes a greater variation than I had supposed.

I have recorded the results of twenty-five observations on the Bessemer flame, most of which were taken at a distance of about thirty feet from the flame, though I have stationed myself at intermediate points between that and the flame, at one time sitting so close as to be almost scorched. Nearly all my observations were made at night, and the lines obtained much better defined than when seen in diffused sunlight.

The record of my observations was kept as follows:—Five columns were ruled, headed—

Degree.	Colour.	Brightness.	Time.	Remarks.
---------	---------	-------------	-------	----------

Note was made of the dark bands as well as the bright ones, both of which were classed according to their distinctness, as very bright, bright, faint, and very faint. In the time-column was

noted the number of minutes after the commencement of the blow at which the lines appear.

At the first two or three observations I attempted to make a thorough note of the changes as they occurred throughout the whole spectrum, but afterward abandoned it as utterly impossible, as at the beginning of the second period the lines come in so fast and the changes are so rapid that they cannot be accurately noted at the exact moment of their occurrence. I therefore confined myself to a few degrees at each observation, and by this method was enabled to note accurately, and at the exact moment of their occurrence, slight changes which otherwise might have escaped notice. Note was also taken of the changes in the general appearance of the whole spectrum during the successive stages of the process. After having made half a dozen observations, while viewing the spectrum of the flame from the converter while it was being heated for another charge, it was discovered that a movement of the eye before the eye-glass occasioned a similar movement of the lines of the spectrum along the scale, on which their position could thus be made to differ more than half a degree. I have seen no notice of this in the statements of others; and it may account for some of the apparent discrepancies. Thereafter, when taking the readings of any of the lines, the position of the eye was so adjusted as to bring the sodium-line exactly at 50° . Owing to the extreme brilliancy of the flame, the aperture may be made exceedingly narrow, and thus the many lines of the spectrum, which with a duller light and broader gauge would be blended together, may be separated.

At the beginning of the blow the spectrum is continuous and very faint, and generally extends from 35° to 120° , covering about three-fourths of the length attained in the second period. This increases slightly in extent and brightness until the appearance of the sodium-line. This line appears at the end of the first period, at the beginning of a more decided flame. It comes flashing through from one extremity to the other for an instant, and then disappears, only to return the next instant in brighter flashes, which are continued for about a minute, by which time the line becomes permanently established. On one occasion the sodium-line, instead of flashing and disappearing as usual, continued visible after a few seconds, and expanded and contracted in width almost isochronously until it became permanently established. The appearance of this line indicates the termination of the first period. This period I have found to vary in extent from three to seventeen minutes in blows lasting from thirteen to twenty-seven minutes. None of the other lines make their appearance in vivid flashes as does the sodium. The lithium-line becomes visible three or four minutes after the first

flash of the sodium. It is very faint at first, but soon becomes quite distinct, and lasts through the blow. The vivid flashing of the sodium-line may be accounted for by the exceedingly small amount of sodium required to produce its spectrum, an amount not exceeding $\frac{1}{180000000}$ of a grain. The slightest momentary combustion taking place in the stream of gas from the converter would at that instant render glowing a sufficient amount of the vaporized sodium to produce its spectrum, and thus occasion the flashes so characteristic in the first appearance of that line. Lithium exists in a much smaller quantity, and requires $\frac{1}{8000000}$ of a grain, or thirty times that given for sodium. By the time the lithium-line is established the red potassium-line at $23\frac{1}{2}^{\circ}$ and, occasionally, the violet line at 135° appear, and the blue and green fields become divided into bands which are so rapidly resolved into bright and dark lines that it is difficult to note the exact time of the appearance of each. The spectrum increases to a dazzling brightness, and extends itself in both directions until it reaches from $23\frac{1}{2}^{\circ}$ to 140° .

During the third period the spectrum becomes more brilliant and the lines more distinct. Several new lines make their appearance in different parts of the spectrum, of which the ones at $51\frac{1}{2}^{\circ}$, 57° , and 67° are well defined, while others are faint and not always visible, some of them appearing only toward the close of the last period. In viewing the lines in the most refracted part of the spectrum, it has been repeatedly observed, both by myself and others, that these lines were more strongly marked when entering the eye at an angle than when viewed directly. That this was not imagination is proved by repeated identification of lines at the same point on the scale.

At the termination of the blow the lines are rapidly swept away, sometimes in the inverse order of their appearance, but more generally they disappear within the space of two or three seconds, leaving a continuous spectrum as at first, though somewhat brighter. Sometimes the sodium- and lithium-lines are swept away with the others, and at other times they remain visible. In either case the change is very decided, and does not generally occupy more than three seconds. In the course of my observations thirty-three lines have been detected, as given in the Table below.

Some of the lines given by Lielegg I have failed to find, but have detected others not given by him.

1st Period : $23\frac{1}{2}$, 35, 50, 135.

2nd Period : $23\frac{1}{2}$, 35, 43, 44, $44\frac{1}{2}$, $45\frac{1}{2}$, 46, $47\frac{1}{2}$, $48\frac{1}{2}$, 50, 52, 53, 56, $56\frac{1}{2}$, $61\frac{1}{2}$, 62, $62\frac{1}{2}$, 63, 65, $66\frac{1}{2}$, $67\frac{1}{2}$, 70, 72, 120, 135.

3rd Period : $23\frac{1}{2}$, 35, 43, 44, $41\frac{1}{2}$, $45\frac{1}{2}$, 46, $47\frac{1}{2}$, $48\frac{1}{2}$, 50, $51\frac{1}{2}$,
52, 53, 56, $56\frac{1}{2}$, 57, $61\frac{1}{2}$, 62, $62\frac{1}{2}$, 63, 65, $66\frac{1}{2}$,
67, $67\frac{1}{2}$, 70, 72, 100, 102, 103, 105, 108, 135.

Among the dark bands detected, the most intense occurred at 44-46, 51-55, 56-58, 62- $64\frac{1}{2}$; others were found at 33- $34\frac{1}{2}$, $36\frac{1}{2}$, $37\frac{1}{2}$, $38\frac{1}{2}$, 40, 68-72.

Many of the dark bands were crossed by bright lines.

I have repeatedly observed the dark band considered by Roscoe to be a hydrogen absorption-line, but have not noticed that its intensity varied with the dampness of the weather. Whether it is an absorption-band or not can be determined by a series of observations continued through wet and dry weather. If this proves to be a hydrogen-line, the Bessemer spectrum will be found more complicated than is generally supposed. It has been thought by some that the dark bands in the spectrum are absorption-lines due to the cooling of the outer sheath of flame; but it is more probable that although the pellets of iron and slag tend to produce a faint continuous spectrum, yet in contrast with the very brilliant lines it appears discontinuous, the dark bands being merely intervals between the bright ones. The iron spectrum has not been satisfactorily identified. It has been suggested that the brightness and size of the lines of the Bessemer spectrum do not allow the iron-lines to appear. In comparing the Bessemer spectrum with Bunsen's spectra of nickel, cobalt, and calcium, no coincidences were observed, except two or three in the latter spectrum. The brightest calcium-line, however, was not visible in the Bessemer spectrum. The Bessemer spectrum contains yet many mysteries to be solved, among which is the cause of the non-appearance of the lines of the spectrum at the beginning and termination of the blow.

This was readily solved when the numerous lines of the spectrum were attributed to carbon; but in proving them to be caused principally by manganese, their disappearance is not so readily accounted for.

One theory to account for it is that the luminous power of the flame is too small at the beginning and end of the process to produce a spectrum. In regard to this, it may readily be shown that the brilliancy of the spectra of incandescent metallic vapours does not depend upon the illuminating-power of a flame, but upon the heat of the flame into which they are introduced. For instance, the spectra are more distinct in the non-luminous flame of a Bunsen lamp than in the ordinary luminous gas-flame. If we take the theory as referring to the feebleness of light given off by those substances in the flame which produce the spectrum, it will resolve itself into the one of change of temperature, not-

withstanding the fact that the illuminating-power of flames of the same temperature varies with the composition of the gas, because there is evidently enough sodium in the flame to give its characteristic line; hence, whatever might be the illuminating-power of the flame, if the heat is sufficiently intense, the sodium-line *will* show itself.

Dr. Wedding adopts the theory that the absence of the spectrum at the beginning and termination of the blow is because the absolute quantity of the bodies volatilized producing the spectrum is at these times too small. His reasons for holding this view are as follows:—"A trace of sodium will give its characteristic line; but, according to Simmler, a much larger quantity of manganese is needed to obtain a recognizable reaction than that which can be detected by the well-known blowpipe reaction with carbonate of soda. Consequently spectrum-analysis does not depend alone upon the *presence* of a body, but also upon the presence of a certain quantity. And although manganese is always left in the iron, it may not be left in sufficient quantity at the termination of the blow to produce the spectrum, and for this reason the lines disappear."

To this theory there are some strong objections. 1st. If we take manganese in sufficient quantity and hold it in a flame, the spectrum will increase in brightness until a uniform temperature is attained; but when the amount of manganese vaporized begins to diminish, its spectrum will gradually decrease in brightness until it disappears. Now, if the disappearance of the manganese-lines in the Bessemer spectrum is owing to the diminution of the quantity of manganese, we should infer that these lines would gradually grow more indistinct and then fade away; but, on the contrary, the manganese-spectrum increases in brilliancy from its first appearance, and is more intense just before being swept away than at any other time. The analysis of the smoke, which appears when the flame ceases, proves that a considerable quantity is still volatilized; and it is notable that in manganiferous iron this quantity increases towards the close of the blow. 2nd. It would be more difficult to account by this theory for the non-appearance of the sodium-line at the beginning of the blow, as sodium then in all probability exists in the issuing gas in sufficient quantity to produce its spectrum at a high temperature, as it is only by special precaution that we can keep it out from any flame. 3rd. A still greater difficulty would arise in applying this theory to the spectra of sodium and lithium at the close of the blow. As has before been stated, these lines sometimes disappear at the moment of complete decarbonization, and sometimes remain. In the former case, to say that our friend sodium had given out would be

doing great injustice to that element, as it has never given us reason for bringing so grave a charge against it. Dr. Wedding, in attempting to demonstrate that the non-appearance of the manganese-lines is owing to the lack of sufficient quantity volatilized to produce its spectrum, makes the following statements :—

From analyses made by Brunner we find that the manganese contained in the iron falls from 3·460 per cent. in the raw material to 1·645, 0·429, and finally to 0·113 per cent. in the decarbonized product, and that the protoxide of manganese in the slag first increases from 37·00 per cent. to 37·90 per cent., and then sinks to 32·23 per cent., and, furthermore, that a certain quantity of manganese is to be found in the smoke. How much manganese is really lost by volatilization cannot be determined, since data are wanting as to the absolute quantity of slag and iron ; consequently we cannot determine how much manganese has been lost by means of the eruptions.

But since the manganese contained in the pig-iron decreases constantly, and that contained in the slag after the termination of the boiling-period also decreases, a considerable volatilization of this body is probable just at the time when the spectrum is best developed. Comparing with this the experiments that can be made in the laboratory, we arrive at the hypothesis that the oxidized manganese which has entered into the slag is not volatilized, but is retained by the slag ; it can therefore get into the flame only in the shape of solid or fluid combinations.

In the above statements the results of the analysis prove that some of the manganese in the slag is volatilized. We cannot consider the manganese-spectrum during the entire process to be due wholly to the volatilization of the manganese directly from the iron ; for while the amount eliminated from the iron grows continually less, the manganese-spectrum grows brighter. Owing to the intimate mixture by the blast of the iron and slag, the manganese oxide contained in the latter is brought into contact with the melted iron and vaporized. This mixing of the slag and iron would cease at the termination of the process ; and this would account for the sudden diminution of smoke.

If there were a sufficient carbonic-oxide flame to render the escaping gases glowing, it is evident they would not issue from the converter as dark smoke, but as incandescent vapour having its characteristic spectrum. The lack of sufficient flame may therefore account for the disappearance of the manganese spectrum. The Bessemer flame presents other problems and opens an intensely interesting field for scientific investiga-

tion; and by the use of more delicate instruments than have yet been employed for this purpose, discoveries may be made which will throw new light upon the subject of spectrum-analysis.

II. *On the Spectra of Carbon.* By W. MARSHALL WATTS, D.Sc., *Physical-Science Master in the Manchester Grammar-School**.

IN the Philosophical Magazine for October 1869 I described and figured a spectrum of carbon, which, so far as I know, had not been previously observed, viz. the spectrum obtained when the spark from an induction-coil is taken in carbonic anhydride or carbonic oxide, and a Leyden jar is included in the circuit. I have recently had the opportunity of mapping the spectrum with a three-prism spectroscope of Browning's.

I have converted the angular readings into wave-lengths by means of an interpolation-curve carefully drawn from exact observation of twenty lines, whose wave-lengths are given in Ångström's map.

The brightest groups of lines are denoted by the Greek letters; and the reading of each line is given on the same scale to which the diagram of my paper already referred to is drawn. The wave-lengths are given in ten-millionths of a millimetre.

Carbon-spectrum No. 4.

α	{	34	Wave-length	6578	
		34.5	"	6532	Coincident with hydrogen α .
		43	"	6165	
		44.5	"	6095	

Two not very bright lines, each triple.

48 Wave-length 5954 Double.

D " { 5895

5889

51 " 5855 Double.

Three faint lines, the first double.

β	{	56	Wave-length	5688	
		56.5	"	5652	
		57.2	"	5640	
		57.5	"	5635	Double.
		62.7	"	5426	Double.
		66	"	5385	Triple.
		69	"	5306	

* Communicated by the Author.

Three faint lines.

{	75	Wave-length	5160	
	75.6	"	5152	
	76.4	"	5140	
	79.7	"	5065	Triple.
	84	"	4969	
	84.2	"	4960	Double.

Faint double line.

{	85.5	Wave-length	4947	
	86.6	"	4927	
	87.3	"	4911	
	88	"	4900	
	89.5	"	4874	
	90.3	"	4860	Double.
{	99.2	"	4730	Double.
	99.6	"	4696	
	101	"	4674	
	102	"	4656	
{	103	"	4646	
	103.5	"	{ 4637 4632	
{	106	"	4590	
	106.5	"	4585	

Five faint lines, of which the fourth is double and the fifth triple.

θ	119.5	Wave-length	4417	Middle of rather wide line.
	123.3	"	4368	Most refracted edge of band.

Perfectly black interval.

	125	Wave-length	4350	Least refracted edge of band.
--	-----	-------------	------	-------------------------------

Two fine lines.

	127	Wave-length	4320	Double.
ι	133	"	4272	Maximum of broad band 130 to 134.

κ	139.8	"	4196	
	140.4	"	4192	

Three faint lines.

	146	Wave-length	4141	
	147.4	"	4130	

Three faint lines.

	152	Wave-length	4092	} Broad band, fine line at 4089.
	to		to	
	154	"	4080	

I have determined also the wave-lengths for two other carbon-spectra.

Carbon-spectrum No. 1.

	42.5	Wave-length	6190
α	44.2	"	6110
	46	"	6050
	47.2	"	5990
	48.5	"	5955
	58	"	5622
γ	60	"	5582
	61.5	"	5534
	63	"	5495
	64.5	"	5463
	66	"	5440
	67	"	5425
	75	"	5170
δ	77	"	5139
	79.3	"	5100
	80.2	"	5082
	97	"	4734
ϵ	98.5	"	4710
	100	"	4689
	101.5	"	4675
	101.7	"	4670
	105	"	4600
ζ	107.5	"	4574
	109.5	"	4550
	110.5	"	4534
	112	"	4514
	113	"	4505
	114	"	4502
		"	

Broad band intersected by a great number of fine dark lines.
Fine bright line.

128 Wave-length 4313 Leastrefracted edge of a broad
band made up of a great number of fine bright lines
separated by dark spaces. At first these lines are too
close to be read; then several bright lines gave the
readings:—

f	4290
	4285
	4279
	4274
	4268
	4261
	4256
	4249
	4243
	4239
	4232

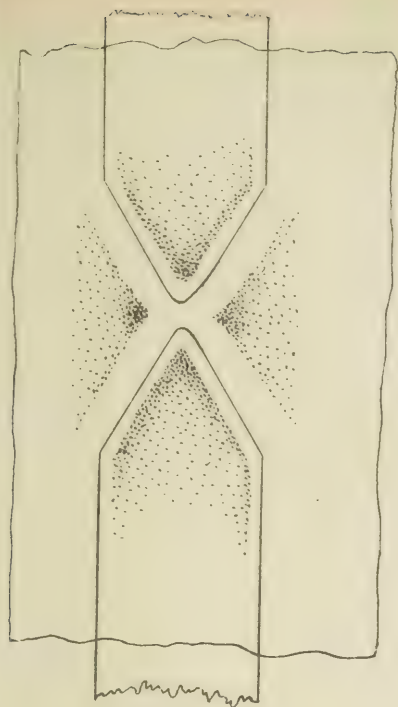
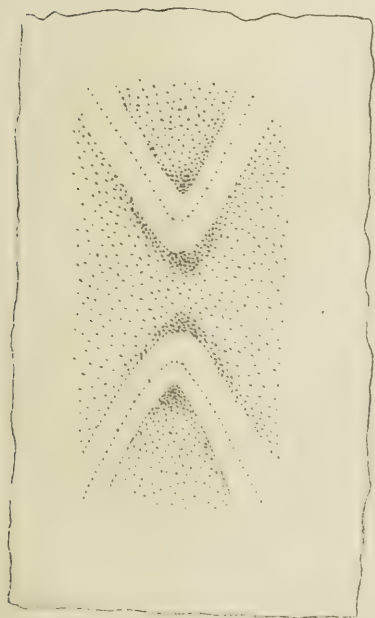


Fig. 1



(a)

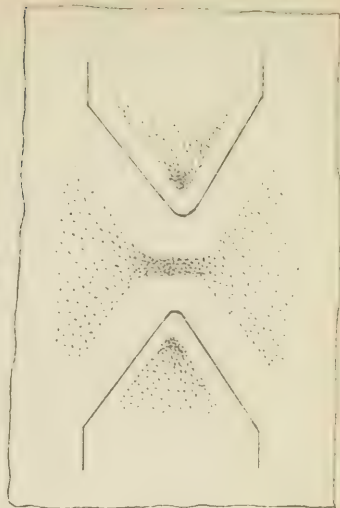
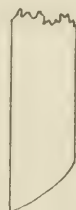


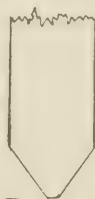
Fig. 3



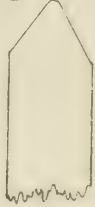
(a)



(b)



(a)



(b)

Fig. 4



Fig. 6



The lines become fainter, but read as far as 4195.

θ	136	Wave-length	4220
	137.5	"	4210
	138.5	"	4190
	140	"	4174
	141	"	4166
	142	"	{ 4160 4158

Each of the bands of which the groups δ , γ , and α consist is made up of an immense number of fine bright lines separated by dark spaces. These lines are closer together on the side towards the red, till where they make up the bright edge of the band the dark lines can no longer be observed.

Carbon-spectrum No. 2.

	40	Wave-length	6060
h	45	"	5803
j	58	"	5602
k	74	"	5195
l	92	"	4834
m	112	"	4505
n	121	"	4395

Each of the bands of this spectrum also is brightest on the least refracted side, and fades away towards the blue. Each band is shaded with dark lines which are closer together at the bright edge—so that the band presents the appearance of a cylindrical pillar with equal flutings, seen at a little distance. The dark lines are not so close together in the band j as they are in the band k .

In conclusion, my best thanks are due to my friend Mr. S. Okell for kindly placing his spectroscope at my disposal.

III. *A few Experiments illustrative of Magnetism and Diamagnetism.* By FREDERICK GUTHRIE*.

[With a Plate.]

THE following few experiments do not exhibit any new facts; but they illustrate known facts in somewhat new forms, and may in some cases be found useful as lecture-experiments.

§ 1. *Magnetism of solid Sulphate of Iron.*—The soft-iron poles of a powerful electromagnet, of the form shown in Plate I. fig. 1, are brought nearly into contact with one another, their upper surfaces being in a horizontal plane. A half sheet of thick cream-laid note-paper is laid upon the poles, and powdered dry sulphate of iron is strewn upon the paper. On tapping the paper, the

* Communicated by the Author.

powder arranges itself immediately above the poles in a manner which need not be represented in a diagram, because the lines of accumulation are identical with the edges of the poles, as in fig. 1*a*. No especial precaution is required for the production of this effect. The powdered crystalline sulphate should be dried for a few seconds above a lamp on a sheet of paper. The effect is shown well if black glazed paper is employed, or a thin sheet of mica or of glass such as is used for mounting microscopic objects. The image of the magnetized sulphate of iron may be thrown on a screen in the usual way. As might be anticipated, no lines indicating lines of magnetic force are exhibited.

§ 2. *Magnetism of solution of Sulphate of Iron*.—This is usually shown by placing the solution in a watch-glass, or on a sheet of mica resting on the two poles. The visible effect is, I think, more striking if the points of the poles, of the shape in fig. 1, are brought within about one eighth of an inch of one another, and two or three drops of the solution suspended between the poles in contact with both (fig. 2*a*). The liquid mass will find a position of equilibrium resulting from its weight, cohesion, and adhesion to the iron. On exciting the magnet, the liquid mass is seen to rise so as to collect around the upper edges of the poles, as in fig. 2*b*. A motion of half an inch can be easily produced in this way. The shadow of the poles and liquid is easily thrown on the screen.

§ 3. *Pseudodiamagnetism of Iron*.—This can be well shown by punching two or three hundred circular disks out of a sheet of tinned iron, punching a hole in the centre of each, piling them one on the top of the other with a disk of paper between each, passing a brass rod down the central holes, and screwing the whole tight with a nut. The whole may be hung on a stirrup of paper. The compound rod presents the appearance of a simple iron cylinder. When at some distance above the poles of a powerful permanent or electromagnet, it assumes, for well-known reasons, the equatorial position. If brought near to the poles it sets axially.

§ 4. *Diamagnetism of Bismuth*.—This effect is usually shown either by the equatorial adjustment of a rod of the metal, or by the simple repulsion from a pointed magnetic pole of a pellet of the metal. The diamagnetism of bismuth may be elegantly shown by bismuth-powder. The powder should be neither too fine nor too coarse. If too coarse, the flat facets of the bismuth crystals hinder motion; if too fine, the powder, though quite dry, is prone to cake together in little hillocks, which collectively resist the individual repulsion of their particles—perhaps for the same reason that the rolling of two wheels on a plane is hindered when the wheels are in contact. Half an ounce

of bismuth may be finely ground in a porcelain mortar for an hour, and the finer portion shaken through a linen bag containing a lump of the metal. The residue which refuses to pass is employed. This is strewn uniformly and not too thickly on a sheet of white glazed paper, which is laid upon the poles of the electro-magnet. On exciting the magnet and gently tapping the paper, figures of accumulation are produced, such as are shown in fig. 3, *a* and *b*. If glass be used instead of paper, the relative position of these lines and the edges of the magnet can be examined. In *a* the poles are nearer together than in *b*. In *a* the continuous lines represent the edges of the poles as seen through the glass. The dotted masses show the distribution of the powder. In *b* the magnetic poles are omitted, but are represented by dotted lines, which consist of finely pencilled ridges of powder accumulated immediately above the edges of the iron poles. When the poles are as near together as they are in *a*, no such lines are seen. These lines disappear on continually tapping the paper or glass, and therefore, I conclude, are not to be attributed to any region of attraction, or to any exceptional structure in the bismuth, but may be compared to the ridge of powder which will rest on the edge of a blunt wedge pointing vertically upwards when powder is strewn on it. Lying, in fact, immediately above the region of maximum repulsion, they are in unstable equilibrium.

If the paper (for the following experiment paper is preferable to glass) supporting the uniformly distributed bismuth-powder be moved horizontally, one or two little ridges of powder are formed (according to the nearness of the poles), which remain stationary as the paper passes beneath them, the particles shifting and rolling so as to maintain the same positions with regard to the poles beneath. With care a whole sheet of paper may be passed beneath. These rolling ridges are best seen when the poles are at a distance intermediate between the distances in *a* and *b*, so that there is only a narrow isthmus of bismuth-powder situated equatorially between the poles, as in fig. 3 *c*. The ridges are the isthmus of powder, and the accumulation *p* or *q* (fig. 3 *c*), according as the paper is moved towards *q* or *p* respectively. I have not found any further structure in the distribution of the bismuth-powder analogous to the structure in the arrangement of particles of iron under similar conditions—no indication, in fact, of lines of diamagnetic force. The bismuth separated from the nitrate by zinc, or from the subnitrate by zinc and sulphuric acid, has not in my hands proved so good for showing these effects as the powdered metal. These diamagnetic phenomena may easily be transferred to the screen in the usual way.

December 3, 1870.

IV. *On the Electromotive Force on the Contact of different Metals.*
By E. EDLUND*.

IN a previous paper† I have proved that if a voltaic current traverses an electromotor in the same direction as the current produced by the electromotor itself, an amount of heat is absorbed in the electromotor which is proportional to the electromotive force multiplied by the intensity of the current. If the current is in the opposite direction, a quantity of heat is produced which is also proportional to the same product. For the same intensity of the current, the quantities of heat absorbed or produced are therefore proportional to the electromotive forces. If a current is allowed to traverse a conducting-wire consisting of two different metals, at the place of contact between them there must be an alteration of temperature, because there exists at that place an electromotive force. If the current goes in the same direction as that caused by the electromotive force, cooling ensues at the place of contact; but there is an increase of temperature in the opposite case. Now experiment has long shown that heating or cooling occurs at the place of contact between two metals when a voltaic current traverses it, and that this alteration of heat, as theory requires, is proportional to the intensity of the current. Hence a measurement of the quantities of heat absorbed or produced on the passage of the voltaic current gives a determination of the magnitude of the electromotive force produced by the contact of the metals. This method of determining electromotive force has the advantage of keeping the circuit closed during the measurement, so that the force is in full activity. The changes of temperature which are produced at the place of contact of two metals by a current passing through them have been frequently investigated; but the magnitude of these changes of temperature depends on the specific heat of the metals, on their conductivity for heat, on the cooling during the experiment, &c., and therefore cannot give a direct measure of the electromotive forces. In the sequel I give the experiments which I have made on this subject in conjunction with Magister Sundell of Helsingfors, and then a comparison of the electromotive and thermoelectric forces of the metals investigated.

2. In the experiments an air-thermometer was used, the bulb of which was a cylinder of very thin sheet copper 125 millims. in length and 80 millims. in diameter. On the outside of each of the two circular ends a copper tube 45 millims. in length was soldered, in such a manner that the tubes were in the centres of

* Translated from Poggendorff's *Annalen*, No. 7, 1870, having been read before the Swedish Royal Academy of Sciences, January 12, 1870.

† Pogg. *Ann.* vol. cxxvii. p. 474. *Phil. Mag.* S. 4. vol. xxxviii. p. 263.

the ends, opposite to each other, and at right angles to the faces. Through these tubes passed the metal wire to be investigated, consisting of two wires soldered together, in such a manner that the place of soldering was just in the middle of the cylinder. On the tubes in question caoutchouc tubes were fitted, which, to make them air-tight, were firmly tied both over the copper tube and over the metal rods. With the same object the space between the wire and the metal tube was filled with melted wax. To preserve the wire from conducting communication with the copper cylinder, the two tubes were lined internally with caoutchouc. At one end of the copper cylinder a metal tube, bent at right angles and provided with a stopcock, was soldered, to which was fitted air-tight a glass tube. This tube was provided with a scale divided into millimetres. The copper cylinder with the glass tube was fastened on a mahogany board, which could be placed, and secured by screws, at different angles with the horizontal. In all the experiments the glass tube formed an angle of 5° with the plane in question. The copper cylinder had such a position that the wire traversing it was horizontal. After the wire to be investigated had been introduced, the cylinder was tested by fixing a small compression-pump to the metal tube and observing the pressure on a water-manometer in connexion with the tube. To keep the temperature around the copper cylinder constant, it was surrounded by a double cylindrical zinc jacket which held 5.85 litres of water. The internal diameter of the zinc jacket was 150 millims., so that the annular space between this and the copper cylinder was 35 millims. wide. The zinc jacket was in four parts, so that it could be conveniently inserted in its position; and suitable apertures were provided for the passage of the wire and of the above-mentioned metal tube. The zinc jacket at the time of the experiments, was filled with water which had the same temperature as the air in the room. In this manner the zinc jacket acquired a temperature which did not materially vary during the short time which was necessary for an experiment. On the further end of the glass tube a short caoutchouc tube was placed, which was connected with an open cylindrical metal box 55 millims. in diameter. Into this box the liquid was poured which was to serve as index in reading off. By raising or lowering the box the column of liquid could be made to stand at any suitable position in the tube; the internal diameter of this tube was about 2.5 millims. When the tube was too narrow, the capillarity acted so strongly that the column of liquid was seriously impeded in its motion, and the air-thermometer thereby became less delicate. In the first four of the subsequent experiments spirit was used as index, and in all the others a coloured liquid containing spirit (cognac).

3. The experiments were made in the following manner. After the wire which was to be investigated had been introduced into the copper cylinder and the whole apparatus suitably arranged, the ends of the wire outside the jacket were connected with the poles of five Bunsen's cells. The intensity of the current was measured with a rheostat. There was moreover in the circuit the usual commutator. To drive part of the air out of the copper cylinder, a more powerful current was used in the first case than that afterwards employed for the proper measurements; thereupon the liquid was poured into the box and the intensity of the current reduced to the magnitude intended for the subsequent experiments. During the cooling the liquid rose in the glass tube; and by raising or lowering the box a suitable position was obtained, which was then permanently adhered to. After the lapse of some time the index remained stationary, and thus showed that the copper cylinder lost just so much heat as was developed by the current in the wire. As in all the experiments the temperature of the copper cylinder but very slightly exceeded that of the surrounding air and of the zinc jacket, it may be assumed, without any material error, that its loss of heat is proportional to its excess of temperature. Although this excess of temperature is unknown, we have for it a reliable relative measure. The index in the glass tube gives the mean temperature of the air in the copper cylinder. That the excess of temperature of the copper cylinder bears to the mean temperature a definite relation, which is independent of the degree of heating, is proved by the experiments themselves. If, as shall immediately be shown, the quantities of heat be determined which are produced or absorbed at the place of contact of the two metals by the passage of the current, it will be found that these quantities, as theory presupposes, are proportional to the intensity of the current. This result would not follow from the experiments, unless it could be assumed that the mean temperature measured by the index bears a definite relation to the excess of temperature of the copper cylinder. After the index has come to rest it gives thus a relative measure for the quantity of heat which the copper cylinder loses during a given time; and this quantity is equal to that which is produced by the current in the same time. Now we assume that while the current continuously traversed the experimental wire, the index has remained at the scale-division a , or that it has advanced $A+a$ divisions from the place which it assumed before the passage of the current. The quantities of heat produced by the passage of the current are of two sorts. Owing to the voltaic resistance, a quantity of heat is produced in the entire wire which is proportional to the square of the intensity. If the intensity of the current

be s , and m be a constant, this quantity of heat will be expressed by ms^2 . On the other hand, according to the direction of the current, an amount of heat is produced or absorbed at the contact which is proportional to the intensity of the current, and may be designated by $\pm ns$, in which n is another constant. We thus find $ms^2 \pm ns = A + a$. If now the direction is reversed, the index after a few minutes is stationary in a new position a' , and we get $ms^2 \mp ns = A + a'$, in which the sign before the second term is altered. From this we obtain $\pm ns = a - a'$. The difference between the two stationary positions of the index when the current first goes in one and then in the other direction thus becomes a measure for the quantities of heat in question.

The above method of observation would furnish very rigorous determinations of the quantities of heat in question if the heating of the air in the copper cylinder were the only cause of the motion of the column of liquid. But this is far from being the case. Variations in the barometric height during even the few minutes which are required to effect two successive readings are frequently sufficient materially to alter the position of the column of liquid. One day, on which a violent storm prevailed, no observations could be made; for, owing to variations in the pressure of the atmosphere, the column of liquid was continually moving backwards and forwards. This, however, is exceptional. To avoid this unpleasantness as far as possible, the experiments were made thus:— After the current had circulated for a sufficient length of time, the position of the column of liquid was observed; the current was then reversed and the new position of equilibrium observed, upon which the current was again reversed and the first observation repeated. Of the first and third readings the mean was taken; and the same course was continued until so many determinations had been obtained that the mean was sufficiently accurate. As the time between two successive observations was approximately equal, accurate results must thus have been obtained, assuming that the barometric pressure varied uniformly. It would not have been impossible to procure accurate corrections for the errors which alterations in the barometric height produce. If, for instance, we had two perfectly equal air-thermometers, one of which, as is here the case, was used exclusively for determining the voltaic heating, and the other for measuring variations in the pressure, by simultaneously reading the two the necessary corrections would be obtained. The same object would also be obtained if the glass tube were not allowed to communicate with the air through the open metallic box, but with a large air-tight closed vessel whose temperature was kept quite constant. Meanwhile the method used was considered adequate to the object in view. We pass now to the proper observations.

4. *Experiment 1.*—The wire experimented with consisted of copper and iron soldered together with tin. The numbers under the head “deflections” are the differences between the two positions of equilibrium of the column of liquid when the current went in opposite directions. These numbers are therefore proportional to the quantities of heat sought. In all the following experiments the copper wire used was taken from one and the same coil. The wire was 1 millim. in diameter. The diameter of the iron wire was about the same. When the positive current went from copper to iron the place of soldering was cooled. The column of liquid, both in this and the three following experiments, consisted of spirit, for which reason they are not accurately to be compared with the later experiments.

Deflections.

19·2

17·5

19·0

Mean . . . 18·6

Intensity of current = tg. 44° 6'.

Experiment 2.—The same wires as before.

Deflections.

11·7

15·5

11·5

Mean . . . 12·9

Intensity of current = tg. 32° 30'.

Experiment 3.—The same wires. The experiment was made on the following day.

Deflections.

17·8

18·0

19·3

21·5

20·3

Mean . . . 19·4

The intensity of the current = tg. 45°.

Experiment 4.—The same wires as before.

Deflections.

13·0

12·5

13·0

12·2

12·0

Mean . . . 12·5

Intensity of current = tg. 31° 25'.

If from these experiments the quantities of heat (x) be calculated on the assumption that, as theory requires, they are proportional to the intensity of the current, we get the formula

$$x = 19.82 \text{ tg. } v,$$

in which v is the angle of deflection of the tangent-compass.

From this we obtain the following calculated values compared with the observed ones:—

	Calculated.	Observed.
Experiment 1. . . .	19.2	18.6
„ 2. . . .	12.6	12.9
„ 3. . . .	19.8	19.4
„ 4. . . .	12.1	12.5

Experiment 5.—The same wires as in the foregoing experiments. The liquid in the glass tube consisted, in this as in all the following experiments, of cognac. The deflections became thereby smaller than before.

Deflections.
18.3
15.8
20.0
16.8
15.5
Mean . . . 17.3

Intensity of the current = $\text{tg. } 44^{\circ} 45'$.

Experiment 6.—The same wires.

Deflections.
11.2
10.7
12.2
11.7
13.5
Mean . . . 11.9

Intensity of the current = $\text{tg. } 33^{\circ} 10'$.

If the last two results be calculated by the formula $x = 17.83 \text{ tg. } v$, we obtain in

	Calculated.	Observed.
Experiment 5. . . .	17.7	17.3
„ 6. . . .	11.7	11.9

Experiment 7.—The wire consisted now of platinum and copper soldered with tin; the former was 1 millim. in diameter. When the positive current went from the platinum to the copper at the place of contact, there was a cooling.

Deflections.

7·5
8·0
7·8
7·0
8·0
7·5

Mean . . 7·6

Intensity of the current = tg. $45^{\circ} 45'$.

Experiment 8.—The same wire, platinum-copper, as in the foregoing experiment.

Deflections.

11·0
12·2
8·0
13·3
9·7
12·0

Mean . . 11·0

Intensity of the current = tg. $55^{\circ} 45'$.

Experiment 9.—This experiment with the platinum-copper wire was made a day later.

Deflections.

7·2
7·0
8·0
6·8
6·5

Mean . . 7·1

Intensity of the current = tg. $45^{\circ} 30'$.

If the mean be taken from experiments 7 and 9, we obtain the deflection 7·4 for the intensity = tg. $45^{\circ} 38'$. The experiments may be calculated from the formula $x = 7·37 \text{ tg. } v$, from which we obtain:—

	Calculated.	Observed.
Experiment 8. . . .	10·82	11·0
„ 7 and 9 . . .	7·54	7·4

Experiment 10.—This experiment was made with a wire of German silver and copper soldered with tin. The German-silver wire was 1 millim. in diameter. When the positive current went from the German silver to the copper at the point of contact, a cooling resulted.

Deflections.

13·2

10·2

9·7

9·7

11·0

10·2

10·2

Mean . . 10·6

Intensity of current = tg. 34° 15'.

Therefore, with an intensity = tg. 45°, the deflection becomes equal to 15·57.

Experiment 11.—In this experiment the copper wire was soldered by means of tin to a bismuth rod 2·5 millims. square. When at the place of contact the positive current went from the bismuth to the copper, cooling resulted.

Deflections.

103·5

107·5

Mean . . 105·5

Intensity of the current = tg. 36° 45'.

With an intensity = tg. 45° the deflection becomes equal to 141·3.

Experiment 12.—The copper wire was soldered to a silver wire 0·6 millim. in diameter. With this combination not the least deflection could be observed. The same was the case with a combination of zinc and copper.

The foregoing experiments give the following electromotor-series, in which, when two successive metals are in contact, the current at the place of contact goes from the preceding to the succeeding—bismuth, argentan (German silver), platinum, copper, and iron. The two metals silver and zinc are so near copper that the experiments left it undecided whether they precede or succeed the last-named metal. The relative numerical values are as follows:—

Bismuth-copper . . .	141·3
Argentan-copper . . .	15·57
Platinum-copper . . .	7·37
Copper-iron	17·83

5. In order to ascertain the thermoelectric relations of these metallic combinations, the following experiments were made. The soldered wires were twice bent near the soldering, so that they were parallel to each other, and the place of contact was inserted in a test-tube which was placed in a large vessel filled

with cold water. In order that the water might be warmed very slowly by the air in the room, the glass beaker was coated with a layer of cotton-wool and covered with a pasteboard disk, through a hole in which the test-tube was inserted. In this test-tube was placed a delicate thermometer, and the space at the top between the wires and the thermometer stopped with cotton-wool. Thus the temperature of the test-tube was kept constant long enough for the place of soldering to acquire the temperature which the thermometer indicated. The free ends of the wires were connected with the conducting-wires of the magnetometer, which were of copper. The connexion between the two different metals was surrounded by cotton-wool, after the bulb of a delicate thermometer had been adjusted as near the place of contact as possible. The deflections of the magnetometer, which were read off in the usual manner with a telescope and scale, were proportional to the intensity of the current. The conducting-power in the circuit for thermoelectricity was measured by means of a magneto-inductor, the coil of which was inserted in the circuit. The deflections which were obtained by the induction-currents caused by the inductor gave thus a measure for the conducting-power.

In what follows I will merely give the mean numbers obtained.

Experiment 13.—Bismuth-copper.

The deflection = 205.5. The difference of temperature at the two places of contact = $12^{\circ}8$. With the magneto-inductor a deflection of 174 divisions was obtained. Hence this number represents the conductivity in the path of the thermoelectric current.

Experiment 14.—Argentan-copper.

Deflection = 61.8. Difference of temperature = $11^{\circ}9$.

Conducting-power = 224.0.

Experiment 15.—Platinum-copper.

Deflection = 22.0. Difference of temperature = $11^{\circ}8$.

Conducting-power = 226.5.

Experiment 16.—Zinc-copper.

Deflection = 2.5. Difference of temperature = $12^{\circ}1$.

Conducting-power = 230.5.

Experiment 17.—Silver-copper.

Deflection = 2.0. Difference of temperature = $13^{\circ}7$.

Conducting-power = 232.0.

Experiment 18.—Copper-iron.

Deflection = 73.0. Difference of temperature = $12^{\circ}8$.

Conducting-power = 228.8.

In these metallic combinations the thermoelectric current went at the warmer place of contact from the first-named metal to the succeeding one.

If the foregoing observations be calculated for a difference of temperature of 10° and a conductivity = 100, we obtain for

Bismuth-copper the deflection	.	.	92.27
Argentan-copper	„	.	23.18
Platinum-copper	„	.	8.23
Zinc-copper	„	.	0.90
Silver-copper	„	.	0.63
Copper-iron	„	.	24.93

In the thermoelectric series the metals are therefore in the following order:—bismuth, argentan, platinum, zinc, silver, copper, and iron. We see from this that the order in the series, which is determined by the heat absorbed or produced on the current passing through the place of contact, is the same as that which is conditioned by the thermoelectric properties of the same metals. In the combinations zinc-copper and silver-copper the quantities of heat were so small that they could not be measured; but we also find that in the thermoelectric series zinc and silver are very near copper, and form with it a very inconsiderable thermoelectric force. In order to compare with one another the numbers obtained, they can be reduced so as to be equivalent for one of these combinations—for example, for copper-iron. We thus obtain the following Table of comparison:—

	Thermoelectric series.	Electromotive series.
Bismuth-copper . . .	92.27	197.6
Argentan-copper . . .	23.18	21.77
Platinum-copper . . .	8.23	10.30
Zinc-copper	0.90	
Silver-copper	0.63	
Copper-iron	24.93	24.93

In the combinations argentan-copper and platinum-copper the numbers in the two series are almost equal, from which it might be assumed that the electromotive and thermoelectric forces are proportional to one another. Yet this is most decisively refuted by the combination bismuth-copper. In order to be quite convinced that this latter combination did not depend upon a faulty measurement of the thermoelectric current, the determination was repeated, by which a deflection of 88.7 divisions was obtained when the difference in temperature of the two solderings was 10° , and the conducting-power was 100. The result was therefore about the same as before. Yet there was a reason for doubting that some error might have crept in in the determination of the numbers for bismuth-copper; for when this pair of wires was removed from the air-thermometer, the bismuth broke near the place of soldering, which necessitated a new soldering before this combination could

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be again investigated. This circumstance might possibly give rise to an error; and hence the experiments with bismuth-copper were repeated.

Experiment 19.—A new bismuth-bar, somewhat thicker than the last, but from the same piece of bismuth, was soldered to the copper, on which the observations were made in the usual manner.

There were thus obtained:—

	Deflections.
	81·0
	73·0
	77·0
	78·5
Mean . .	77·4

Intensity of the current = $g. 29^{\circ}$.

If from this the deflection be calculated for an angle of 45° , it is found to be = 139·6, thus almost equal to the number previously obtained (141·3).

The magnitude of the thermoelectric current with the new bismuth-copper combination was next determined, as well as, for comparison, those with three of the combinations previously mentioned. So great a resistance from the rheostat was now interposed in the circuit, that the resistance could in all the experiments be regarded as quite equal. It is only necessary to adduce the mean numbers suitably reduced in order to facilitate inspection and comparison with the preceding. In this manner it was found that the thermoelectric currents for these combinations were as follows:—

Bismuth-copper	78·47
Argentan-copper	24·17
Platinum-copper	8·30
Copper-iron	24·93

These numbers, within the limits of errors of observation, are equal to those previously obtained, except that for bismuth-copper, with which combination the deflection was smaller than before. This is doubtless due to the new bismuth-bar being somewhat different from the first. Yet it is seen that the exception which the combination bismuth-copper makes does not depend upon a faulty determination.

According to Seebeck, the various metals have the following order in the electric-tension series:—silver, platinum, copper, iron, bismuth, and zinc, in which each preceding in contact with the succeeding is electronegative. Although different observers have found the order different, partly in consequence of the

metals being impure, it is clear that there is no similarity between the electric-tension series and that which the metals form as regards the heat produced or absorbed on the passage of a current. As, then, from the theoretical proofs which we gave in a former paper, the quantities of heat in question furnish a measure of the electromotive forces, it must be concluded that the electric-tension series is in no close connexion with these forces. Hence it is highly probable that the electrical tension does not exclusively depend on the contact between the two metals, but on the layer of gas or of water which is condensed on their surfaces—a view in favour of which many reasons might be urged. On the other hand, it was seen that the thermoelectric series is the same as that for the electromotive forces. The metals which in contact with each other produce the greatest electromotive force, also produce the most powerful thermoelectric current when the place of contact is heated; but these thermoelectrical currents are not in all combinations proportional to the corresponding electromotive forces.

V. On the Heat Developed in the Combination of Acids and Bases.
—Second Memoir. By THOMAS ANDREWS, M.D., F.R.S.,
Hon. F.R.S.E., Vice-President of Queen's College, Belfast*.

IN a paper communicated to the Royal Irish Academy in 1841, I gave an account of a large number of experiments on the heat disengaged when acids and bases, taken in the state of dilute solution, enter into combination, and when bases, insoluble in water, are dissolved in dilute acids. The following general conclusions or laws were deduced from those experiments:—

Law 1.—The heat developed in the union of acids and bases is determined by the base, and not by the acid—the same base producing, when combined with an equivalent of different acids, nearly the same quantity of heat, but different bases different quantities.

Law 2.—When a neutral is converted into an acid salt, by combining with one or more atoms of acid, no change of temperature occurs.

Law 3.—When a neutral is converted into a basic salt, by combining with an additional proportion of base, the combination is accompanied with the evolution of heat†.

Three years later I laid before the Royal Society of London the results of an experimental investigation of the heat developed when one base is substituted for another in chemical compounds.

* From the Transactions of the Royal Society of Edinburgh, Session 1869–70. Communicated by the Author.

† Transactions of the Royal Irish Academy, vol. xix. p. 228. [Phil. Mag. Sept. 1841, p. 183].

The law deduced from this inquiry is implicitly involved in the foregoing, of which it may indeed be regarded as a necessary consequence. It was enunciated in the following terms:—

Law 4.—When one base displaces another from any of its neutral combinations, the heat evolved or abstracted is always the same, whatever the acid element may be, provided the bases are the same*.

Finally, the law of metallic substitutions, first announced in the *Philosophical Magazine* for August 1844, was thus stated in a paper published in the *Philosophical Transactions* for 1848.

Law 5.—When an equivalent of one and the same metal replaces another in a solution of any of its salts of the same order, the heat developed is always the same; but a change in either of the metals produces a different development of heat.

In 1845 [1844] a paper appeared by Graham on the heat disengaged in combinations, the second part of which refers to the heat produced when hydrate of potash is neutralized by different acids†. The results arrived at by this distinguished chemist exhibit a close agreement with those contained in my first communication to the Royal Irish Academy.

The concluding part of the elaborate memoir of MM. Favre and Silbermann on the heat disengaged in chemical actions is chiefly devoted to the same subject. A large number of experiments are described, which are nearly a repetition of those I had previously published. Their results bear a general resemblance to those given by myself in 1841; but they widely differ in the details. The authors of this able memoir fully recognize the accuracy of my fourth law, which asserts the equality of thermal effect when one base is substituted for another. “M. Andrews,” they observe, “avait en effet établi que, quel que soit l’acide d’un sel, la quantité de chaleur dégagée par la substitution d’une base à une autre pour former un nouveau sel est la même, lorsque l’on considère les deux mêmes bases”‡.

In a preceding paragraph of the same memoir, the authors object to what they conceive to be my first law, and state that it is not in accordance with the results of their investigations. As the question is one of some importance, I may perhaps be permitted to quote the passage in the original language. “Ses conclusions, savoir: que la chaleur dégagée par l’équivalent d’une même base combinée aux divers acides est la même, ne s’accordent pas avec les résultats de nos recherches, et ne nous paraissent pas pouvoir être admises.” No doubt, through inad-

* *Philosophical Transactions* for 1844, p. 21.

† *Memoirs of the Chemical Society*, vol. ii. p. 51. [*Phil. Mag.* June 1844, p. 401.]

‡ *Ann. de Chim. et de Phys.* S. 3. vol. xxxvii. p. 497 (1853).

vertence, MM. Favre and Silbermann have here given an inaccurate statement of my first law. It did not declare that precisely the same amount of heat is disengaged by all the acids in combining with the same base, but that the heat is determined by the base, "the same base producing, when combined with an equivalent of different acids, *nearly* the same quantity of heat." A comparison of the results of MM. Favre and Silbermann with those in my original memoir will show that I had fully recognized and described the deviations from the other acids, exhibited, on the one hand, in excess, by the sulphuric acid, and on the other, in deficiency, by the tartaric, citric, and succinic acids. "If we refer," I remarked, in the original memoir of 1841, "to the first, second, and fourth Tables, as being the most extensive, from the large number of soluble compounds formed by potash, soda, and ammonia, it will be observed that the sulphuric acid develops from $0^{\circ}8$ to nearly 1° more than the mean heat given by the other acids; while the tartaric, citric, and succinic acids fall from $0^{\circ}4$ to $0^{\circ}55$ short of the same. A minute investigation of the influence of the disturbing sources of heat will no doubt discover the causes of these discrepancies. The high numbers for sulphuric acid are probably connected with that acid's well-known property of developing much heat when combining with successive atoms of water. All the other acids develop nearly the same amount of heat in combining with the same base, the greatest divergences from the mean quantity being, in the case of potash, $+0^{\circ}24$ and $-0^{\circ}13$; in that of soda, $+0^{\circ}26$ and $-0^{\circ}14$; and in that of ammonia, $+0^{\circ}17$ and $-0^{\circ}05$. These differences are almost within the limits of the errors of experiment"*.

But although there is a superficial agreement between my original results and those of MM. Favre and Silbermann, they will be found, when examined closely, to differ widely in detail, and on points of great importance. I had found that the oxalic acid disengages almost exactly the same amount of heat in combining with the soluble bases as the hydrochloric, nitric, and many other mineral acids; and this observation I have always regarded as one of the main foundations of Law I. MM. Favre and Silbermann, on the contrary, have inferred from their experiments that "the following organic acids—the oxalic, formic, valeric, and citric—disengage sensibly the same quantity of heat, but it is less (*plus faible*) than that given by the foregoing mineral acids"—among which they enumerate the nitric and hydrochloric. According to my experiments, no distinction of this kind can be admitted between acids derived from the mineral and organic kingdom, inasmuch as the oxalic acid develops at

* Transactions of the Royal Irish Academy, vol. xix. p. 240.

least as much heat in combining with the bases as the hydrochloric, nitric, and several other strong mineral acids.

The experiments to be described in this paper were made some years ago; but their publication has been deferred from accidental circumstances. I have, however, recently repeated a few of the more important of them with a slightly modified form of apparatus. The solutions were taken in so dilute a state that the heat disengaged never exceeded $3^{\circ}\cdot5$ C. A standard solution of sulphuric acid was prepared and carefully analyzed, by precipitating a given weight with a soluble salt of barium, and weighing the sulphate of barium. The strength of the alkaline solutions was adjusted with great care by means of this standard acid. The same solution of each alkali was employed in all the experiments, and the quantity used in each experiment was determined by careful weighing. The acid solution was of such a strength that, after being mixed with the alkali, an excess of two or three per cent. of acid was present. The alkaline solution was contained in a light glass vessel, in which a large platinum crucible holding the acid was carefully floated. By giving a rapid rotation, by means of a light stirrer, to the acid solution in the platinum crucible, a perfect equilibrium of temperature was soon established between the two liquids. The initial temperature of the solutions was usually about $1^{\circ}\cdot5$ below that of the air, and the final temperature of the mixture about $1^{\circ}\cdot5$ above it. The corrections for the heating and cooling action of the surrounding medium were determined with great care. The mechanical process of adding the acid to the alkaline solution produced no change of temperature; and as the heat disengaged in the combination raised the liquid almost instantly to the maximum temperature, the whole correction required was for cooling. The first temperature was read one minute after the addition of the acid to the alkaline solution, the mixture being stirred during the whole of that time. If δ represents the correction, and ϵ the excess of temperature above the air in Centigrade degrees, the value of δ will be given by the following expression:—

$$\delta = \epsilon \times 0^{\circ}\cdot012.$$

As a proof of the accuracy of the method of mixture adopted in this inquiry, I may mention that, being desirous to know whether the dilute acids employed in these experiments produced any change of temperature when mixed with water, I made the experiment with nitric acid by the method just described, substituting water for the alkaline solution, with the unexpected result of a fall of $0^{\circ}\cdot01$. On varying the conditions of the observation, so as to obtain a larger effect, it was ascertained not only that a diminution of temperature had actually occurred, but

that the observed fall represented approximately its true amount. When hydrochloric acid of equivalent strength was diluted to the same extent, an elevation of temperature of $0^{\circ}05$ was produced.

The accuracy of experiments of this kind, where the whole thermal effect observed amounts only to 2° or 3° , depends greatly on the thermometer employed. Unless its indications are perfectly trustworthy in every part of the scale, the labour of the inquirer will only end in disappointment. I have therefore taken every precaution to secure this important object. The tube of the thermometer was calibrated and divided with care, according to an arbitrary scale, by means of a dividing-instrument contrived for the purpose, and provided with a short screw of great accuracy, made by Troughton and Simms. The divisions, etched finely on the glass, corresponded to about $0^{\circ}05$ C.; and the readings could be made with certainty to less than $0^{\circ}01$. The division of the scale corresponding to 0° was determined from time to time in the usual way; and another point, about 30° C., was fixed by comparison with four other thermometers similarly constructed, whose scales extended from the freezing- to the boiling-point of water. The readings of these four instruments, when reduced to degrees, rarely differed from each other within the limits to which they could be read, or $0^{\circ}02$. The reservoir of the thermometer used in these experiments was 75 millims. long, and, when immersed in the liquid, occupied nearly its entire depth.

As some uncertainty always exists with regard to the thermal equivalent of glass vessels, I made two sets of comparative experiments—one with a thickly varnished copper vessel, and the other with a vessel of platinum. The mean result of these experiments coincided almost exactly with the result obtained when the glass vessel was employed.

The weight of the glass vessel which contained the alkaline solution was 58 grms., and corresponded thermally to 11.4 grms. of the solutions formed. The thermal equivalent of the reservoir of the thermometer and of the stirrer was 0.9 gm. The alkaline solution weighed 160 grms., and contained the equivalent of 1.738 gm. of SO^3 . The acid solution weighed 42.5 grms. Hence the entire thermal value of the apparatus, in terms of the solution formed, was:—

Solution	grms. 202.5
Glass vessel	11.4
Thermometer and stirrer	0.9
		<hr/> 214.8

A correction (additive) of $\frac{1}{240}$ was made to the direct readings
Phil. Mag. S. 4. Vol. 41. No. 270. Jan. 1871. D

for the mercury in the stem of thermometer. The results are given to thousandths of a degree; but this apparent minuteness is due to the reduction of the indications of the arbitrary scale to degrees.

In the following detailed statement of the experimental results; Inc. is the increment of temperature observed, corrected for the mercury in stem, and δ is the correction for cooling.

Potash and Sulphuric Acid.

Inc.	3.358	3.356	3.366
δ	.010	.024	.021
	<u>3.368</u>	<u>3.380</u>	<u>3.387</u>
Mean inc. corrected	. 3.378		

Potash and Nitric Acid.

Inc.	2.971	2.976	2.977
δ	.018	.019	.017
	<u>2.989</u>	<u>2.995</u>	<u>2.994</u>
Mean inc. corrected	. 2.993		

Potash and Hydrochloric Acid.

Inc.	3.004	3.002	3.005
δ	.017	.019	.017
	<u>3.021</u>	<u>3.021</u>	<u>3.022</u>
Mean inc. corrected	. 3.021		

Potash and Oxalic Acid.

Inc.	3.036	3.048	3.040
δ	.017	.017	.016
	<u>3.053</u>	<u>3.065</u>	<u>3.056</u>
Mean inc. corrected	. 3.058		

Potash and Acetic Acid.

Inc.	. . .	2.835	2.846
δ016	.007
		<u>2.851</u>	<u>2.853</u>
Mean inc. corrected	. 2.852		

Potash and Tartaric Acid.

Inc.	2.707	2.717	2.730
δ	.014	.014	.013
	<u>2.721</u>	<u>2.731</u>	<u>2.743</u>
Mean inc. corrected	. 2.732		

Soda and Sulphuric Acid.

Inc.	. . .	3.322	3.335
δ025	.024
		<u>3.347</u>	<u>3.359</u>
Mean inc. corrected	. 3.353		

Soda and Nitric Acid.

Inc.	. . .	2.914	2.919
δ012	.012
		<u>2.926</u>	<u>2.931</u>
Mean inc. corrected	. 2.929		

Soda and Hydrochloric Acid.

Inc.	2.963
δ019
		<u>2.982</u>
Increment corrected	. 2.982	

Soda and Oxalic Acid.

Inc.	. . .	3.029	3.013
δ019	.020
		<u>3.048</u>	<u>3.033</u>
Mean inc. corrected	. 3.040		

Soda and Acetic Acid.

Inc.	. . .	2.816	2.812
δ017	.018
		<u>2.833</u>	<u>2.830</u>
Mean inc. corrected	. 2.831		

Soda and Tartaric Acid.

Inc.	. . .	2.693	2.693
δ019	.015
		<u>2.712</u>	<u>2.708</u>
Mean inc. corrected	. 2.710		

Ammonia and Sulphuric Acid.

Inc.	2°·967	2°·959
δ	·017	·010
	2°·984	2°·969
Mean inc. corrected .	2°·976	

Ammonia and Nitric Acid.

Inc.	2°·556	2°·551
δ	·010	·015
	2°·566	2°·566
Mean inc. corrected .	2°·566	

Ammonia and Hydrochloric Acid.

Inc.	2°·609	2°·607
δ	·015	·015
	2°·624	2°·622
Mean inc. corrected .	2°·623	

Ammonia and Oxalic Acid.

Inc.	2°·635	2°·630
δ	·015	·016
	2°·650	2°·646
Mean inc. corrected .	2°·648	

Ammonia and Acetic Acid.

Inc.	2°·469	2°·482
δ	·017	·016
	2°·486	2°·498
Mean inc. corrected .	2°·492	

Ammonia and Tartaric Acid.

Inc.	2°·365	2°·354
δ	·017	·016
	2°·382	2°·370
Mean inc. corrected .	2°·376	

In the following Table I have collected the foregoing results, arranging the acids in the order of their thermal action:—

Acid.	Potash.	Soda.	Ammonia.
Sulphuric acid	3°·378	3°·353	2°·976
Oxalic acid	3°·058	3°·040	2°·648
Hydrochloric acid	3°·021	2°·982	2°·623
Nitric acid	2°·993	2°·929	2°·566
Acetic acid	2°·852	2°·831	2°·492
Tartaric acid	2°·732	2°·710	2°·376

It is interesting to observe how closely the results in the three vertical columns agree relatively with one another. The acids follow in the same order under each base; and even the differences in the amount of heat disengaged by the several acids in combining with the different bases approximate in many cases closely to one another. Thus the heat given out when the sulphuric acid combines with potash exceeds that given out when the oxalic acid combines with the same base by 0°·320, the corresponding differences in the case of soda and ammonia being 0°·313 and 0°·328. If, in like manner, we compare the differences between the heat disengaged by the acetic and tartaric acids, we fall upon the numbers 0°·120, 0°·121, and 0°·116. Even in the case of the oxalic, hydrochloric, and nitric acids, which disengage so nearly the same amount of heat, the same order is observed with the three bases. It must be particularly

remarked that the oxalic acid disengages from $0^{\circ}022$ to $0^{\circ}058$ more heat in combining with these bases than the hydrochloric acid, and from $0^{\circ}065$ to $0^{\circ}111$ more than the nitric acid. The conclusion of MM. Favre and Silbermann, that the organic acids (oxalic, formic, acetic, &c.) disengage sensibly less heat than the mineral acids, is thus entirely disproved, and the original results recorded in my work of 1841, according to which the oxalic acid disengages at least as much heat as the nitric, phosphoric, arsenic, hydrochloric, hydriodic, boracic, and other mineral acids (with the exception of the sulphuric acid), are fully confirmed. The tartaric, citric, and succinic acids, it is true (as was also shown in the same work), give out about $\frac{1}{4}$ less heat than the average of the other acids; but the acetic and formic acids fall scarcely $\frac{1}{30}$ below the mean, and the oxalic acid is always above it. These results, in all their main features, are fully corroborated by the experiments recorded in this paper, which were performed with a more perfect apparatus and a more exact thermometer than I had at my command in my earlier investigations. A reference to the same paper will show that while acids differing so widely from one another as the oxalic, phosphoric, arsenic, nitric, hydrochloric, and boracic acids scarcely present any sensible difference in the quantities of heat which they disengage in combining with the bases, and while, of the other acids examined, the sulphuric acid (and probably also the sulphurous acid) presents an extreme deviation of about one-eighth above the mean, and the tartaric-acid group a deviation of about $\frac{1}{30}$ below it, the bases, on the contrary (and the subsequent researches of Favre and Silbermann have confirmed this result), differ altogether in thermal power from one another. Thus equivalents of the oxides of magnesium and of silver give out $4^{\circ}1$ and $1^{\circ}8$ of heat respectively in combining with nitric acid, the former oxide having therefore 2.3 times the thermal power of the latter. Yet, as is well known, both these bases fully saturate the acid, and the resulting solutions are even neutral to test-paper. For these reasons I have no doubt whatever that the first law, as enunciated in 1841, is the expression of a true physical law, and that in the combination of acids and bases in presence of water the heat disengaged is determined by the base and not by the acid. It is true that, in this as in similar physical inquiries, experimental results cannot immediately be obtained free from complication or disturbing influences. The same remark applies to the experimental proof of the great law discovered by Dulong and Petit, which connects the specific heats and atomic weights of the elementary bodies, and also to that of the remarkable relations discovered by Kopp between the composition and boiling-points of many organic

liquids. We have already seen an illustration of one of these disturbing influences, in the fact that dilute nitric acid, when mixed with water, gives a slight fall of temperature, hydrochloric acid a rise; and the differences of specific heat in the solutions formed will, to a small extent, modify the results. But the cause of the higher thermal power of sulphuric acid I have not been able to discover; and future researches must decide whether it depends upon some disturbing cause, or (which is less probable) upon its possessing an exceptionally high thermal power. One condition, however, is essential, or Law 1 will not apply. The acid and base must be capable of combining when brought into contact, and of forming a stable compound. In the paper so often referred to, I showed that hydrocyanic acid and potash, which fail to fulfil this condition, do not disengage the normal amount of heat when mixed; and the same observation will doubtless be found to apply to a large number of metallic oxides, which form unstable compounds with, and imperfectly neutralize, the bases.

As regards the experimental proofs of the other laws, even those of the fourth law, the truth of which is admitted by MM. Favre and Silbermann, they are only approximative; and here also we meet occasionally with peculiar and unexpected results. Thus a slight fall of temperature occurs, as Hess showed long ago, in the conversion of the neutral sulphate of potash into the acid salt; and I found, as indeed might have been expected from their alkaline reaction, that in the conversion of the ordinary phosphates and arseniates into super-salts, a disengagement of heat occurs amounting to about one-seventh of that disengaged in the formation of the salts themselves. In other cases results, at first view startling and apparently anomalous, will be found to be strictly in accordance with the general principles already laid down. In the formation of double salts there is no disengagement of heat—a principle announced in 1841, and which ought perhaps to be enunciated as a distinct law, although it is implicitly involved in Law 2. Again, if tribasic phosphoric acid or arsenic acid is added in fractional portions to a solution of potash till the subsalts are formed, the heat disengaged on each addition of acid corresponds to the amount of acid added; but after this point has been reached, the disengagement of heat follows a different law. The pyrophosphoric acid, on the other hand, behaves in the same way as the nitric acid and most other acids, when added in successive portions to solutions of potash or soda, equal increments of heat being evolved for equal additions of acid, till the pyrophosphate of potash or soda is formed*.

* Transactions of the Royal Irish Academy, vol. xix. pp. 245–248. The

Appendix.

In the following Tables I have given the results described in this communication and those of 1841 in a form which admits of comparison with one another, and with those of MM. Favre and Silbermann. I have also added a few determinations recently made by M. Thomsen of Copenhagen*. It will be seen that the original experiments of 1841 exhibit, on the whole, a fair agreement with those now communicated to the Society. From the small scale on which they were performed (the whole weight of the solutions after mixture being less than 30 grms.), the imperfect form of the apparatus, and the uncertainty of the thermometric indications, I have indeed been surprised to find them so near the truth. The results of MM. Favre and Silbermann do not exhibit the precision which might have been expected from the high character of those experimentalists, and from the accuracy of other parts of their great work. The mercurial calorimeter employed by them appears to have been little adapted to its purpose; but after making due allowance for its imperfections, I am at a loss to account for the serious errors into which they have fallen. M. Thomsen's experiments have evidently been made with care, and his results agree comparatively with my own; but the absolute amount of heat obtained by him falls far short of what I have found. It is indeed much easier to obtain results relatively than absolutely correct. The numbers given in this paper will, I believe, be found rarely to differ *relatively* more than $\frac{1}{200}$ from the truth; but they may hereafter require a small correction in respect to their *absolute* value. That correction, however, can scarcely be more than $\frac{1}{30}$ of the whole amount; and I have little doubt that the number, for example, given by Thomsen to express the heat disengaged in the combination of soda with nitric acid will prove to be as far below the true number as that given by MM. Favre and Silbermann is above it.

TABLE I.—Potash.

Acid.	Andrews, 1841.	Favre and Silbermann.	Andrews, 1870.
Sulphuric	16330	16083	16701
Nitric	15076	15510	14800
Hydrochloric	14634	15656	14940
Oxalic.....	14771	14156	15124
Acetic.....	14257	13973	13805
Tartaric	13612	13425	13508

observations of Graham confirm the statement that no heat is evolved in the formation of any double salt. *Memoirs of the Chemical Society*, vol. i. p. 83.

* Poggendorff's *Annalen*, vol. cxxxviii. p. 78.

TABLE II.—Soda.

Acid.	Andrews, 1841.	Favre and Silbermann.	Andrews, 1870.	Thomsen.
Sulphuric	16483	15810	16580	15689
Nitric	14288	15283	14480	13617
Hydrochloric	14926	15128	14744	13740
Oxalic.....	14796	13752	15032	
Acetic.....	14046	13600	14000	
Tartaric	13135	13651	13400	

TABLE III.—Ammonia.

Acid.	Andrews, 1841.	Favre and Silbermann.	Andrews, 1870.
Sulphuric	14135	14690	14710
Nitric	12440	13676	12683
Hydrochloric	12440	13536	12964
Oxalic.....	12684	13088
Acetic.....	12195	12649	12316
Tartaric	11400	11744

VI. *On a curious Property of Gun-cotton.**By* Dr. L. BLEEKRODE*.

WHILST engaged in some experiments wherein I tried to ignite gun-cotton with the electric spark, I thought to facilitate its explosion by wetting it with a highly inflammable liquid, as, for example, the bisulphide of carbon [CS²]. But it was only this latter substance that was immediately set on fire by the spark—while the gun-cotton did not explode, but remained apparently intact amidst the burning bisulphide, presenting almost the aspect of a mass of snow slowly melting away.

This experiment may be easily repeated by moistening gun-cotton with bisulphide of carbon, ether, benzine, or alcohol, and igniting it afterwards with some flame or other; all these liquids yield the same results; and it is without danger, even if great quantities are used.

This effect is not due to the presence of water, as is proved by the fact that benzine or the bisulphide of carbon, which may be easily had without any trace of water, act in the same manner as ether or alcohol; but the explanation of this singular behaviour of so eminent an explosive substance as gun-cotton belongs to the very interesting researches of Professor Abel “On the Combustion of Gun-cotton and Gunpowder”†, from which I

* Communicated by the Author.

† Proceedings of the Royal Society, 1863–64, vol. xiii. p. 214.

make the following quotation:— “These results indicate that if, even for the briefest space of time, the gases resulting from the first action of heat on gun-cotton upon its ignition in open air are impeded from completely enveloping the burning extremity of the gun-cotton twist, their ignition is prevented; and as it is the comparatively high temperature produced by their combustion which effects the rapid and more complete combustion of the gun-cotton, the momentary extinction of the gases, and the continuous abstraction of heat by them as they escape from the point of combustion, render it impossible for the gun-cotton to continue to burn otherwise than in the slow and imperfect manner, undergoing a transformation similar in character to destructive distillation.”

In the same paper are described numerous experiments to prove the accuracy of this statement; and these were made with compact gun-cotton twist burning in the open air, in a vacuum, or enclosed in narrow tubes; I think that the combustion when the cotton is moistened with CS_2 or any other volatile substance proves it in a most direct and very satisfactory manner. By enclosing a small quantity of the moistened substance in the aperture of a glass tube open at both ends and then igniting it, some of the gases resulting from the destructive distillation may be collected, among which nitrous acid is easily detected by its characteristic odour.

It was also remarked by Professor Abel that gunpowder, when ignited under the same conditions as gun-cotton, behaves in a different way: for example, a piece of gunpowder weighing 14 grains was placed in an apparatus wherein the pressure of air was equal to 0.65 inch of mercury; when in contact with a wire heated to redness it emitted first vapours of sulphur, and after the lapse of three minutes the powder deflagrated. The same facts present themselves when gunpowder is spread on a disk and moistened with CS_2 ; as it burns, the gunpowder does not immediately explode, but some time afterwards; yet it is always a true explosion, caused by the chemical action between the charcoal and the fused saltpetre, whilst the sulphur is already sublimed*.

The above-described experiments afford a striking illustration of latent heat for the lecture-table; and I may add in this respect, that when a small piece of phosphorus is placed in the moistened mass of gun-cotton, during the combustion this substance is melted, and even boils, but does not burn, as it is prevented from contact with oxygen till all the gun-cotton has disappeared.

And, to conclude, it may be perhaps of use to remark that, by keeping gun-cotton in a flask under a layer of benzine or

* Professor Abel, *loc. cit.* p. 212.

bisulphide of carbon, the danger of an explosion in the case of fire is eliminated; for if the liquid happen to be ignited by some cause or other, the gun-cotton will be only slowly destroyed. When required for use, a brief exposure to air restores its explosive properties.

The Hague, December 1870.

VII. *On Solutions for depositing Copper and Brass by means of Electric Force.* By W. H. WALENN, F.C.S.*

THE practical value of an electro-deposit mainly depends upon its structure. As this structure is formed under circumstances that have no analogy in the ordinary treatment of metals by heat, it is worthy of separate study. When a metal is cast into the desired form, the homogeneity of the result is, to a great extent, ensured by the whole of the metal being in the same state at the same time; the electro-deposit, on the contrary, takes time, and frequently has to bear the influence of fluctuating electric power in its production, besides other irregularities that none can realize except those who have studied them and have submitted the resulting deposit to rigorous examination.

The variety of structure of deposits of metals and alloys from their electrolytical solutions is very great, and is sometimes much affected by slight alterations and impurities in the solution, to an extent not generally known. To obtain trustworthy results in the examination of deposits, it is necessary to observe minutely the surface of the metal as soon as it comes from the solution, as well as the fracture of the deposit, with a comparatively low magnifying-power, a single lens being sufficient.

The deposits from the solution of a neutral salt of a metal, or of a neutral salt with an excess of its acid radical, are well known, and may be classified into the "powdery" (great electric power upon a weak solution giving a *black* powder with the principal metals), the "reguline," and the "crystalline." The reguline deposit is subject to variation in quality; in the case of copper, perhaps the best solution to deposit the normal quality is:—

1. *Normal solution.*—Cupric sulphate, one pound; sulphuric acid, one pound; water, one gallon.

This solution deposits the metal in a compact mass, quite solid, and having an exterior surface somewhat botryoidal in character.

On one occasion the normal solution was worked by means of a porous cell without a separate battery (the single-cell process),

* Communicated by the Author.

and the zinc solution was allowed to infiltrate considerably into the external cupric solution; the result was that the botryoidal form entirely disappeared, the metal being deposited in tufts close together (when viewed by the naked eye) and rising vertically from the surface of the cathode. These, when viewed with a microscope, were seen to be perfectly separate tufts standing up at right angles to the surface. The influence of a small proportion of zincic sulphate to the gallon of depositing solution, as recommended by Napier* (one ounce to the pound of cupric sulphate), is known to nearly annul the botryoidal form, and to render the deposit tough, compact, and even; but, to test the action of an excess of this salt, a solution was made as follows:—

2. *Abnormal solution*.—Cupric sulphate, one pound; sulphuric acid, one pound; zincic sulphate, four ounces; water, one gallon.

On trial with a Maynooth cell, this solution yielded a result like in character to that produced by the infiltration of the zinc solution from the porous cell, but more marked.

The general characteristic of electrobrassing-solutions, when in good order, is to deposit the metal in needles at right angles to the surface, more or less detached from one another, according to the state of the solution and the electric power employed. This peculiarity can be traced to the fact of alkaline solutions being used for this purpose, the metal of the alkali being prone to be deposited (for an infinitely small duration of time) together with the heavy metals, and to the hydrogen copiously given off during deposition. With Morris and Johnson's solution † (the solvent solution of which is composed of potassic cyanide and ammoniacal sesquicarbonate) it was found impossible to deposit a thickness of metal of more than .01 inch free from spongy or tree-like formation. Other solutions were found to give a like result. Although in facility of management and constancy of action the author found that a solvent solution composed of potassic cyanide and neutral ammoniacal tartrate ‡ was superior to the other known brass solutions, this solution would not deposit good metal of more than .03 inch thick, hydrogen being given off (in less quantity than in other solutions) during deposition.

Having observed that a solution containing the sulphates of the metals, together with potassic cyanide and an excess of ammonia, could be worked with a single-cell arrangement (the porous cell containing the same materials as the external cell without the copper and zinc) without the evolution of hydrogen, the author found that this was owing to the presence of the oxides of the metals, as well as their cyanides, in the solution; and,

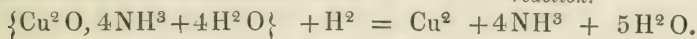
* Electro-metallurgy. By James Napier, F.C.S. Fourth edition, p. 49.

† Morris and Johnson's Patent-Specification, No. 1032, A.D. 1852.

‡ Walenn's Patent-Specification, No. 1540, A.D. 1857.

after many experiments, he has adopted the plan of adding to any brass electrolytic solution (but preferably to that containing potassic cyanide and ammoniac tartrate, mentioned above) the oxides of the constituent metals as well as their cyanides, and, if this does not perfectly stop gaseous evolution, some cupric ammonide*. The resulting solution has been found to produce brass of a uniform character; and it gives a deposit which, when microscopically examined, is in texture very much like that produced by the acid copper solution containing zinc, recommended by Napier; the solution is capable of depositing the alloy to any thickness. If combined oxygen is carried to the cathode by the cupric ammonide, the action may be expressed by the equation

At the cathode before chemical reaction. *At the cathode after chemical reaction.*



Cupric ammonide. Hydrogen. Copper. Ammonia. Water.

The formula for cupric ammonide is that of Malaguti and Sarzeau. The cupric ammonide and the eliminated hydrogen go to the cathode, chemical reaction then takes place, copper is deposited, ammonia is in solution, and water is formed.

The imperfect results afforded by ordinary brass solutions are :

1st. The porous character of the deposit, arising from the separation between the parts of the needle-shaped structure.

2nd. The constant and brisk evolution of hydrogen, being favoured by the pointed character of the deposit from its commencement, augments as the coating grows in thickness, and gives in a short time, and at the attainment of an insignificant thickness, a tree-like formation, which still further degenerates as the coating proceeds, so as to render the accomplishment of any thickness more than a mere film impracticable. In practice, the article being coated is taken out every few minutes, scratch-brushed, and then replaced. With two hours of this treatment, the coating is still porous and is very little better than that given by a skilful application of bronze-powder, and then it has to be varnished (when used for coating iron) to prevent rust-spots forming on its surface by the action of the air.

3rd. The evolution of hydrogen consumes electric power, and renders a high intensity requisite to make up for the loss; so that, with ordinary solutions, two Bunsen's cells, of considerable effective positive surface, are barely sufficient to bring out the article in a serviceable condition.

4th. It is very difficult, and in some cases impossible, to prevent the exudation of some of the solution occluded within the pores. This exudation causes unsightly spots, which are only apparent after the work has been completed some hours.

* Walenn's Patent-Specification, No. 3930, A.D. 1868.

The results achieved by the total prevention of the evolution of hydrogen, by the author's process, are :—

1st. The attainment of a perfectly solid coating with no pores.

2nd. The ability to deposit to any thickness without any deterioration of coating, not only rendering the external protective coating of varnish quite unnecessary, but making it easy to coat a given article to any thickness without removing it from the bath until it is finished. If desired, the coated article may be pickled in brass-finisher's pickle, or it may be burnished, filed, or otherwise treated as solid brass, to produce a serviceable article.

3rd. There being no evolution of hydrogen during deposition, all the electric force is utilized in depositing metal, and no more than one Wollaston's or Smee's cell is necessary to give a perfect result. To save time in practice, it is often advisable to use a single Bunsen's or Maynooth cell, or equivalent magneto-electric power.

4th. The solidity of the coating prevents the possibility of any exudation or formation of spots, for there are no pores to occlude the solution.

Many square feet of cast and wrought iron have been coated, and many varieties and different qualities of brass have been produced, by the author's process. Amongst other work publicly exhibited is a cast-iron roller weighing ninety-six pounds and having a coating of twenty-nine pounds.

The iron articles coated by the new process have all the strength of iron and the beauty of brass, in consequence of the complete command obtained thereby over the quality of the deposit, as well as its thickness and perfect structure.

74 Brecknock Road, N.
December 1870.

VIII. *On the Probable Character of Cometary Orbits.* By A. S. DAVIS, B.A., *Mathematical Master, Leeds Grammar-School.*

[Continued from vol. xl. p. 190.]

IN a previous paper on this subject I arrived at the conclusion that out of every hundred comets approaching the sun in hyperbolic orbits, and having perihelion distances greater than the radius of the earth's orbit, more than twenty-three on the average will have excentricities greater than 1.02.

The assumptions on which this result was obtained were, first, that the parallaxes of the stars nearest the sun are about half a second; and secondly, that the velocities of comets at a great distance from the sun vary from zero to three radii of the earth's

orbit per annum, and that between these values there are as many comets with one velocity as with another. Let us consider whether these assumptions require modification.

First, with regard to the assumption as to the parallax of the nearest stars, it is to be remarked that there are only two stars known to have parallaxes as great as half a second. Hence in only two directions from the sun are we certain that the distance of the nearest stars is as small as that which corresponds to a parallax of half a second.

But the greater the distances of the nearest stars are assumed to be, the less will be the percentage of comets with excentricities greater than 1.02. It will be well, in order to avoid all possibility of obtaining too high a percentage through assuming too small a value for the distance of the nearest stars, to calculate what the percentage would be on the extreme assumption that their parallax is only $\cdot 05''$. (As the sphere which would enclose all the stars whose parallaxes are greater than $\cdot 05''$ will be 1000 times as large as the sphere which encloses the two stars whose parallaxes are greater than $\cdot 5''$, it is extremely improbable that a large number would not be met with within this sphere in all directions from the sun.) The result of calculation on this assumption is that, of the comets whose perihelion distances are unity, the average percentage having excentricities greater than 1.02 lies between 19 and 23, the first of these values being the percentage calculated when R is taken equal to 400,000, and the second when R is taken equal to 4,000,000.

Secondly, with regard to the velocities which comets have on first coming into the sun's attraction, we remark that the smaller these velocities are assumed to be, the smaller will be the percentage of comets with excentricities greater than 1.02. We must then inquire whether we have assumed these velocities to be greater than they really are.

Now, as I have before remarked, the average velocity of comets before coming under the predominant attraction of the sun must be as great as the average velocity of the fixed stars. Let us consider, then, what evidence we have regarding the velocities of the stars.

I. The velocities of the stars relatively to the sun arising from the sun's motion in space is about 1.6 per annum.

II. The sums of the squares of the proper motions of the stars before and after correction for the sun's motion differ by only a small fraction of the whole uncorrected sum. The explanation of this fact appears to be that the average proper motion of the stars is much greater than the sun's proper motion.

III. When the parallax as well as the proper motion of a star is known, the component of its velocity perpendicular to the line

in which it is seen may be ascertained. Dividing the proper motion by the parallax, this component is obtained expressed in terms of the radii of the earth's orbit per annum.

The following Table exhibits the components of the velocities thus calculated of the stars which are believed to have a sensible parallax.

Star.	Parallax.	Proper motion.	Component of velocity.
α Centauri	0.9187	3.9	4.2
61 Cygni.....	0.5638	5.12	9.1
21250 Lalande	0.2709		
17415 Eltzen.....	0.247		
0830 Groombridge...	0.226	6.97	30.3
70 Ophiuchi	0.16	0.25	1.6
α Lyrae	0.155	0.33	2.1
Sirius	0.150	1.34	9.0
ϵ Ursæ Majoris	0.133	0.54	4.1
Arcturus.....	0.127	2.22	17.1
Polaris	0.067	0.03	2.3
Capella	0.046	0.44	9.6

The average of these velocities is much higher than the greatest value we assumed for the whole velocities of comets at a great distance. It must, however, be noticed that many of these stars were selected as likely to exhibit a sensible parallax because of their large proper motions, and therefore that their average velocity cannot be taken altogether as an index of the average velocity of all stars.

IV. More than 300 stars are catalogued as having proper motions greater than $0''.5$. If we assume, what will certainly be the fact, that most of these stars have parallaxes much less than $0''.2$, then most of them have proper motions much greater than 2.5 .

V. By the displacement of the lines in the spectrum of the light of Sirius, it has been found that it is approaching the sun with a velocity of 29.4 miles a second. This is a velocity of 10.14 radii of the earth's orbit per annum.

For these reasons we may I think conclude that the velocities of the stars, and therefore of comets, when at a great distance from the sun, are on the average greater than the velocities which we have assumed comets to have in our calculations, and, therefore, that the percentage of hyperbolic comets having excentricities greater than 1.02 resulting from our calculations will be less than the actual percentage.

We have hitherto only considered those comets which approach the sun in hyperbolic orbits. It is, however, quite possible that comets may approach the sun for the first time, coming from the

interstellar regions of space and passing away never to return again, in orbits which in the neighbourhood of the sun are elliptical, but which lose their elliptical character in those parts which are so distant from the sun that the attraction of the stars is not insensible compared with the sun's attraction. This will be the case for any comet whose relative velocity with respect to the sun when first it comes under its predominant attraction is less than that due to a fall from an infinite distance—that is, if R be its distance from the sun, if it is less than $\sqrt{\frac{2\mu}{R}}$.

With regard to the probable number of comets moving in such elliptic orbits compared with the number of comets moving in hyperbolic orbits, it may be remarked that though the number of comets having at a great distance from the sun velocities relatively

to the sun less than $\sqrt{\frac{2\mu}{R}}$ will be very small compared with the whole number of comets, yet these comets are more likely to be drawn near the sun and thus to be observed from the earth than comets with larger velocities, and, therefore, that the number of comets with such elliptic orbits and perihelion distances so small that the comets may be observed from the earth may not be small compared with the number of hyperbolic comets. I will now attempt to calculate what this number will probably be.

Let us consider a comet at a distance R so great that the direction in which it may be moving is wholly independent of the sun's position with respect to it. That this may be the case, we must take R so great that the attractions of the stars upon the comet may be as great as the sun's attraction, and therefore the resultant attraction upon the comet not necessarily in the direction of the sun. Let us consider first what would be the subsequent motions of comets if, upon arriving at the distance R , they were to cease to be influenced by any attraction except that of the sun. We shall take into consideration those comets only whose subsequent orbits would be elliptical—that is, those comets which have velocities less than $\sqrt{\frac{2\mu}{R}}$.

Putting $e = 1 - \epsilon$, it will be seen, by making the proper alterations in formula (8) of my former paper, that the probable number of comets which have perihelion distances lying between q and $q + \delta q$, and excentricities lying between 1 and $1 - \epsilon$, is proportional to $-\log\left(1 - \frac{R\epsilon}{2q}\right) \times \delta q$.

The greatest value ϵ can have will be that which it has when R is itself the aphelion distance of the comet's orbit; and this

value of ϵ is given by the equation

$$\frac{R}{q} = \frac{2-\epsilon}{\epsilon}; \therefore \epsilon = \frac{2q}{R+q}.$$

Substituting this value in the expression above, we obtain

$$-\log \frac{q}{R+q} \times \delta q = \log \frac{R}{q} \times \delta q \text{ very nearly.}$$

Thus, on the supposition that after comets have arrived at a distance R from the sun they cease to be attracted by the stars, the probable number of comets having perihelion distances between q and $q + \delta q$ and excentricities less than unity is proportional to $\log \frac{R}{q} \times \delta q$. But the attraction of the stars upon a comet does not cease when it has arrived at a distance R from the sun. The effect, however, of their attraction will be to diminish the number of comets with perihelion distances so small that they can be observed from the earth; for their attraction will generally diminish the force acting upon the comet in the direction of the sun, and will cause the direction of the whole resultant attraction not necessarily to lie in the direction of the sun. Thus the comets will have their directions of motion less deflected towards the sun than they would be if they were not attracted by the stars, and consequently will have larger perihelion distances, so that fewer will be observed from the earth.

We may then take $\log \frac{R}{q} \times \delta q$ as a superior limit to the number of comets with perihelion distances between q and $q + \delta q$, and excentricities less than unity. We have, therefore, the ratio of elliptic to hyperbolic orbits less than

$$\log \frac{R}{q} \times \delta q : \log \left(1 + .23 \times \frac{R}{2} \right) \times \delta q,$$

a ratio which for moderate values of q does not differ much from unity.

Hence out of every hundred comets approaching the solar system for the first time and having perihelion distances equal to unity, about fifty will move in hyperbolic orbits, and of these fifty about ten or eleven at least will have excentricities greater than 1.02. For comets with perihelion distances larger than unity there will be even a larger percentage than ten or eleven having excentricities greater than 1.02.

We are thus led to the conclusion that a large majority of those comets whose orbits are undistinguishable from parabolas are not visitors to the solar system for the first time, because, if they were, the percentage of comets with excentricities greater than 1.02 would be very much smaller than by theory it should be.

I now wish to point out a possible method by which comets of very long period may have become permanently attached to the solar system.

The known elliptic comets are supposed to have had their orbits changed into ellipses by perturbations produced by the planets; and there are several reasons for believing this to be the true account. Thus the planes of their orbits have a decided tendency to make a small angle with the plane of the ecliptic, and a large percentage of them have a direct motion. Both these facts agree well with the theory that they have had their orbits changed by the perturbing influence of the planets.

It may be well here to remark that this tendency of the planes of the orbits of periodic comets to have a small inclination to the plane of the ecliptic does not arise from any disturbing action of the planets upon their orbits after they have become periodic, but arises from the fact that a non-periodic comet is more liable to become changed into a periodic comet the smaller its inclination to the ecliptic upon its first approach to the sun. The disturbing influence of the planets upon the planes of the orbits of periodic comets will on an average act as much to increase as to diminish their inclinations to the ecliptic. But when we come to consider the parabolic comets, we find that the comets having a retrograde motion are more numerous than those which have a direct motion, and also that the planes of their orbits exhibit no tendency to make a smaller inclination with the plane of the ecliptic than with any other plane. Out of 174 parabolic comets, 101 or 58 per cent. have a retrograde motion, and 102 or 59 per cent. have the planes of their orbits inclined at an angle greater than 45° to the ecliptic. These facts are opposed to the theory that these comets have had their orbits changed by the attractions of the planets; and we are thus induced to seek for some other cause.

May not an adequate cause be found in the resisting medium which is supposed to surround the sun? We have seen that many comets must approach the sun for the first time moving in elliptic orbits. The aphelion distances of some of these orbits will be less than the distance of the nearest stars, and may be so small that, if they be diminished to any considerable extent, the comets will become permanently attached to the sun. Now the effect of a resisting medium upon a very excentric orbit in changing the length of the major axis will be much greater in the course of one revolution than it is in the case of an orbit of moderate excentricity such as Encke's. We may form a rough notion as to what effect the resisting medium would have in diminishing the semiaxis of the orbit of a comet of long period by the following approximate calculations.

Assuming with Encke that the resistance opposed to a comet's motion varies as the square of its velocity, and inversely as the square of its distance from the sun, the change produced by the resisting medium at any part of the comet's orbit is given by the equation

$$\frac{da}{du} = -k \frac{(1+e \cos u)^{\frac{3}{2}}}{(1-e \cos u)^{\frac{5}{2}}},$$

where e is the excentricity, u the excentric anomaly, and k a constant.

For very small values of u we have

$$\begin{aligned} \frac{da}{du} &= -k \left(1 + e - \frac{eu^2}{2} + \dots\right)^{\frac{3}{2}} \left(1 - e + \frac{eu^2}{2} - \dots\right)^{-\frac{5}{2}} \\ &= -k \frac{(1+e)^{\frac{3}{2}}}{(1-e)^{\frac{5}{2}}} \left\{1 - \frac{e}{1+e} \cdot \frac{u^2}{2} + \dots\right\}^{\frac{3}{2}} \left\{1 + \frac{e}{1-e} \cdot \frac{u^2}{2} - \dots\right\}^{-\frac{5}{2}} \\ &= -k \frac{(1+e)^{\frac{3}{2}}}{(1-e)^{\frac{5}{2}}} \left\{1 - \frac{3}{4} \cdot \frac{e}{1+e} \cdot u^2 + \dots\right\} \left\{1 - \frac{5}{4} \cdot \frac{e}{1-e} \cdot u^2 + \dots\right\} \\ &= -k \frac{(1+e)^{\frac{3}{2}}}{(1-e)^{\frac{5}{2}}}, \text{ neglecting terms containing } u^2 \text{ and } \frac{u^2}{1-e}. \end{aligned}$$

Hence

$$\int_{-u}^{+u} \frac{da}{du} du = -2k \frac{(1+e)^{\frac{3}{2}}}{(1-e)^{\frac{5}{2}}} \cdot u,$$

if u be very small. The excentric anomaly of the end of the latus rectum is $\tan^{-1} \frac{b^2}{a^2 e} = \frac{1-e^2}{e}$ very nearly, if e does not differ much from unity.

Hence the change produced in the semiaxis of the orbit whilst the comet is passing from one end of the latus rectum through perihelion to the other end is $-2k \frac{(1+e)^{\frac{3}{2}}}{e(1-e)^{\frac{5}{2}}} = -\frac{8k \sqrt{2}}{\epsilon^{\frac{3}{2}}}$ nearly, if $e=1-\epsilon$ and ϵ is small.

The change produced in another orbit will be $-\frac{8k \sqrt{2}}{\epsilon'^{\frac{3}{2}}}$; and

the ratio of these changes is $\left(\frac{\epsilon'}{\epsilon}\right)^{\frac{3}{2}}$. Let us suppose that the two orbits have equal perihelion distances. Then, since a very large portion of the whole change which is produced during one revolution in the semiaxis of the comet's orbit is produced whilst the comet is passing from one end of the latus rectum, through

perihelion, to the other end, the above ratio may be taken to represent approximately the ratio of the changes produced in the two orbits during one revolution.

If a, a' be the semiaxes of the two orbits, and $\delta a, \delta a'$ the changes which they undergo in one revolution,

$$\frac{\delta a}{\delta a'} = \left(\frac{\epsilon'}{\epsilon} \right)^{\frac{3}{2}}.$$

If $P'^2 = a'^3$, so that P' is the periodic time in an orbit whose semiaxis is a' , then

$$\delta a' = \frac{2}{3} \frac{P'}{a'^{1/2}} \cdot \delta P';$$

$$\begin{aligned} \therefore \frac{\delta a}{a} &= \frac{2}{3} \left(\frac{\epsilon'}{\epsilon} \right)^{\frac{3}{2}} \cdot \frac{P' \cdot \delta P'}{a a'^{1/2}} \\ &= \frac{2}{3} \cdot \frac{\epsilon'^{\frac{3}{2}} \cdot \sqrt{a} \cdot P' \cdot \delta P'}{q^{\frac{3}{2}} a'^{1/2}}, \end{aligned}$$

if q is the common perihelion distance of the two orbits. If, now, P', a' , &c. belong to Encke's comet,

$$\epsilon' = .153, \quad a' = 2.216, \quad P' = 3.296, \quad \delta P = \frac{0.11}{365}, \quad q = 0.340.$$

Let us now take $a = 200,000$, so that the aphelion distance is 400,000, or about half the distance of a star whose parallax is $0''.25$. On substitution and reduction we obtain

$$\frac{\delta a}{a} = .018.$$

Thus, on the assumption that, under the same circumstances as to velocity and distance from the sun, the resisting medium would offer the same amount of resistance to the comet of long period as to Encke's comet, we arrive at the conclusion that its aphelion distance will be diminished by about .018 of its original length.

A diminution to this amount would be sufficient to change a comet's motion in some cases into a periodical revolution about the sun. There are, however, reasons for believing that the retardation which comets of long period have undergone on first becoming attached to the solar system must have been greater than the retardation which Encke's comet now experiences. The first reason is, that a comet appears to lose at each approach to the sun its rarest and most volatile parts, or those which offer the greatest resistance to motion; and the second is, that

at the period when it first became attached to the solar system, the nebulosity about the sun was probably much greater than it now is.

Lastly, we have already noticed that, out of 174 parabolic comets, the number which have a retrograde motion is considerably greater than the number which have a direct motion, and also that the number which have an inclination to the ecliptic greater than 45° is considerably greater than the number which have an inclination less than 45° .

If these facts are not due to mere chance (and that they are due to chance is improbable), they are themselves an evidence that the parabolic comets are not, for the most part, visitors to the solar system for the first time*. But if these comets are periodic, and if they have become so by some cause which has acted equally on comets approaching the sun with direct and retrograde motions and in any directions, then we should expect that the comets with a retrograde motion would be more numerous than those which have a direct motion, and also that those whose planes are inclined to the ecliptic at an angle greater than 45° would be more numerous than those whose planes have an inclination less than 45° . This we should expect, because a greater number of those with a direct motion than of those with a retrograde motion and a greater number of those with an inclination less than 45° than of those with an inclination exceeding 45° would be subsequently changed by the attractions of the planets into comets of comparatively short periods; and therefore, of those left unchanged, a majority would have retrograde motions and large inclinations to the ecliptic. It is a confirmation of this theory to find that, if we take into account all known comets, 48·9† per cent. and 51·1 per cent. respectively, or numbers very nearly equal, have direct and retrograde motions.

The sum of what I have endeavoured to prove is:—

I. That the great majority of comets whose orbits are undistinguishable from parabolas are moving in elliptic orbits, and are permanent members of the solar system.

II. That these comets have not, like the comets of short period, become permanently attached to the solar system through changes which have been produced in their orbits by the perturbing attractions of the planets; and that the loss of velocity which they have experienced in passing through the resisting

* The probability that out of 174 comets not more than 73 will have direct motion is about $\frac{1}{20}$.

† For this and other statements see Chambers's 'Astronomy,' pp. 346–349).

medium which surrounds the sun may have been sufficient in many cases to change the paths of comets which would otherwise have passed away to other stars, into periodical orbits about the sun.

Roundhay Vicarage,
December 1, 1870.

Errata in former paper.

Page 183, line 20, *for* directions *read* their directions of motion when at a great distance from the sun.

Page 189, line 20, *for* 1'' *read* 0''·05.

IX. *Note on the Zodiacal Light.*

By RICHARD A. PROCTOR, B.A.*

IT cannot but be regarded as a remarkable circumstance that the nature of the zodiacal light should in the present state of astronomy continue to be a *quæstio vexata*. I do not here refer to the physical constitution of this object, respecting which we may possibly be unable for many years to form a satisfactory theory, but to the determination of the actual position of the zodiacal light in space. Astronomers have been able to determine from geometrical considerations the paths of such objects as comets and meteors; it would therefore seem that the position of such an object as the zodiacal light ought ere this to have been determined.

Yet it must be admitted that there are peculiar difficulties in this problem. We can reason respecting the distance and motion of a comet, because we know that our observations are made on one and the same body, whose motions are in accordance with the laws of gravity. It is otherwise with respect to the zodiacal light. We see a certain glow or radiance occupying a definite position with respect to the horizon and to the celestial circles; but we have no means of ascertaining whether the objects from which that radiance proceeds are the same at any one time as at any other, or indeed (as will presently appear) whether a single one of the constituents forming the zodiacal gleam at one season is present within the same region of the solar system at another.

The geometrical conditions applicable to the zodiacal light are, however, too definite to admit of question; in other words, the path to be followed in seeking for a theory of this object is unmistakable. Hitherto, so far as I am aware, that path has not been traced out *far enough* for the attainment of definite views, the perplexities which presently surround us as we follow it having seemed perhaps to render further research hopeless.

It happens, however, not unfrequently that the very difficul-

* From the Monthly Notices of the Royal Astronomical Society, November 11, 1870.

ties surrounding a subject of this sort assist us—in this way, that they enable us to reject theories which otherwise might engage our attention and so cause perplexity. Precisely as the very complexity of a lock makes us all the more certain that a key which opens the lock is the key really appertaining to it, so, where a subject of astronomical research presents many perplexing phenomena, these become so many reasons the more for accepting a theory which is not contradicted by any one of them.

This is, I think, the case with the zodiacal light. We are able, as I hope now to show, by considering the peculiarities of this object to get rid, one after another, of various theories which might otherwise distract our attention. And though by this process of elimination we may not be enabled to determine quite the true theory of this object, we can yet considerably narrow the field within which selection has to be made.

The first considerations to be dealt with are those which depend on the normal features of the zodiacal light. It is well known that the light exhibits usually the figure of an oblique conoid whose axis lies close by the ecliptic, and whose vertex lies at a varying distance from the position of the sun. Near the axis the light grows brighter, except close by the vertex, where it is even fainter than at the other parts of the border. The following Table, prepared by Herr Klein from modern observations, indicates the varying range of the vertex from the place of the sun, though it must be remembered (and will be recognized at once by every one familiar with the varying position of the ecliptic during the year, and other like circumstances) that these measures indicate variation in the extent of visibility rather than (of necessity) any real variation in the extent of the light.

Day of the year.		Distance of vertex from sun.	Part observed.
January	2	83°0	Western half.
„	26	91°8	„
February	11	81°0	„
March	14	74°0	„
April	14	75°0	„
May	4	65°0	„
August	1	77°0	„
September	15	58°0	Eastern half.
October	17	74°5	„
November	12	71°3	„
„	29	56°5	„
December	13	61°0	„
„	28	80°5	„

The setting of the zodiacal light when the western half is visible, and the rising of the light when the eastern half is visible, take place quite regularly, and in a manner precisely corresponding with what would be observed if the zodiacal light were a distant object like a planet, a star, or a portion of the Milky Way.

Now these circumstances at once enable us to reject the theory that the zodiacal light is a terrestrial appendage, by which I understand for the moment an object lying within the earth's atmosphere. For there can be no question whatever that if any definite portion of our atmosphere were rendered luminous in any way, that portion would either occupy an unchanged position or would shift according to the laws regulating the process of illumination, or according to the winds or other like terrestrial causes. Now that on any given occasion such causes might so operate as to give the illuminated air the appearance of rising or setting, as celestial objects do (that is, not *merely* rising or setting, but rising or setting along declination-parallel), is quite possible, however unlikely. To take an illustrative instance: a balloon seen at any one instant between an observer and the sun might be carried by the winds so as to continue between him and the sun, even until the hour of sunset. But to suppose that night after night at any station a relation so peculiar would characterize the illuminated air, is like supposing that a balloon, started day after day from a given place, would day after day fulfil the condition considered above. This is obviously incredible. But even if it were credible, it would be insufficient, since the region of our atmosphere which would have to be illuminated, in order to account for the zodiacal light as seen in one place, would, as seen from other stations, present an appearance wholly different from that of the zodiacal light. In fact, if the former place were in England, the zodiacal light would actually be in the zenith of places 900 miles or so west or east of England.

Next we have the normal aspect of the zodiacal light in different latitudes to consider. Now we have the most positive assurances from astronomers of eminence that the zodiacal light, wherever seen, occupies ordinarily precisely those regions of the heavens corresponding to the theory that it is too far from the earth to have an appreciable parallax displacement. We have the evidence of practised astronomers like the Astronomer Royal for Scotland, Captain Jacob, and others; and all the evidence we have points to the conclusion that the zodiacal light, as seen in the tropics, extends at any moment over those same parts of the stellar heavens which it illuminates as seen from our northern stand-point. The *limits* of the light may seem greater in those

latitudes than in ours; but the axis of the conoidal gleam is situated precisely as with us.

Now it seems wholly unquestionable that this quality of the light should dispose at once and for ever of the theory that the zodiacal light is due to the existence of a ring of matter around the earth.

Let it be remembered that there is only one way in which the ordinary aspect of the zodiacal light can at all be interpreted on such an hypothesis. If there were a ring of meteorites as far from us as the moon is, then undoubtedly there would be a gleam in the west after sunset, and in the east before sunrise, in the position where we see the zodiacal light. And further, the individual meteorites producing any portion of that gleam would undoubtedly rise and set much as the zodiacal light is observed to do. But there would also be a gleam, and a much brighter gleam, in the south. The meteorites rising and setting would turn only a small portion of their illuminated faces towards us, those in the south (on or close by the ecliptic) would be "full," so to speak, and their combined lustre would be proportionately more considerable. Now supposing the ring exactly coincident with the ecliptic, the earth's shadow would fall on the part due south. But the width of this shadow would (on the supposition we are considering) be relatively small. At midnight, in our latitudes, we should undoubtedly, on this supposition, see two arms of light extending from the eastern and western horizon along the ecliptic, each growing brighter and brighter towards the south; and a relatively narrow black rift would lie between the bright extremities of these arms. It is no theory that this would be the case, but a simple deduction from the most obvious geometrical laws.

If, then, we are to have a ring round the earth, it must lie far within the moon's orbit, so that the earth's shadow may be wide enough to cover the meteorites along the whole of that long arc which under ordinary circumstances is undoubtedly unilluminated. The earth's shadow cannot be more than 8000 miles across *anywhere*, and we must have our ring at such a distance that this width of 8000 miles may correspond to (or subtend) that wide arc of darkness actually observed under ordinary circumstances. (It is absolutely essential that ordinary circumstances should be accounted for; only when this has been done need we begin to inquire into extraordinary circumstances.)

Now we need not leave our own latitudes to decide how far off the ring should be to account for the apparent dimensions of the zodiacal light, because on the theory that the earth's shadow falling on a ring of some sort defines the limits of visibility of

the light, it would follow, precisely as in the case just considered, that the light would grow brighter and brighter up to the very edge of the shadow. (Supposing that edge to correspond to the extent of the earth's shadow, there would be a somewhat ruddy bordering; but up to the commencement of that fringe there would be a regular increase of brilliancy.) But passing over this consideration (and also the consideration that the observed aspect of the zodiacal light in our latitudes is wholly inconsistent with the aspect thus shown to be due to the hypothesis we are dealing with), we may take as most favourable to the hypothesis of a meteoric ring near the earth those observations of the zodiacal light in tropical regions which give to the ordinary apparitions of the light the greatest observed extension from the sun.

We have it on the authority of Professor Piazzzi Smyth that, even when he observed the zodiacal light under exceptionally favourable conditions (from an elevation, namely, of no less than 11,000 feet above the sea-level), the western tongue had completely set fully four hours before the eastern tongue began to rise. Now, even if the eastern tongue were just beginning to rise when the western tongue had fully set, there would still be an arc of 180° between the two vertices*. But the shadow of the earth would not account for such an arc as this between the vertices, unless the outer part of the ring had a radius not exceeding $\sqrt{2} \times$ radius of the earth (even in the most favourable case of a station near to the equator); and with such a radius as this the outer part (even) of the ring would be always invisible from places having a higher northerly or southerly latitude than 45° .

And even if we set this demonstration on one side for a moment, it is yet obvious that, whether a ring lying relatively near the earth coincided in plane with the equator or with the ecliptic, or with any intermediate plane, it could not possibly exhibit any approach to coincidence with the celestial ecliptic when viewed from high latitudes. Further, as seen from high northern latitudes, such a ring would always have a parallactic displacement, causing it to lie to the south of its geocentric position, and *vice versa*; whereas no such association between the latitude of the observer and the apparent position of the zodiacal light has ever been observed, far less such a systematic association as the case requires.

It is geometrically impossible, then, that the ordinary aspect of the zodiacal light can be accounted for by any theory which represents it as due to a ring of light-reflecting bodies around

* It must be remembered that each vertex, as the zodiacal light was seen by Professor Smyth, lay close by the ecliptic.

the earth, whether that ring be close by the earth or at a distance comparable with the moon's*.

We need not consider the theory that the light may be due to a self-luminous ring around the earth, for obvious reasons.

Now, passing from the normal features of the ring to more or less exceptional peculiarities, we find ourselves compelled to reject yet one other theory of the light; I mean the theory that it is due to a disk of minute bodies travelling in orbits of small eccentricity around the sun.

The peculiarities which oppose themselves most strikingly to this theory are those which relate to the position and extent of the zodiacal light, though it will be obvious that the observed variations in the apparent brightness of the light are not readily explicable on this hypothesis.

Admitting the existence of a disk of bodies travelling as supposed, it will be evident that the changes affecting the motions of any member of the system would correspond exactly to those which would affect the motions of any considerable orb travelling at a similar distance from the sun. In other words, the changes would resemble those slow periodic changes which affect the orbits of the earth, Venus, and Mercury. Nor is it conceivable that the members of the system would so interfere with each other's motions as to affect appreciably at any time the appearance of the disk. Now changes such as these, affecting the individuals of a set of bodies which at any one time were spread with a certain uniformity (as the ordinary appearance of the zodiacal light would imply to be the case with its constituents), could not account for the observed changes in the position and extent of the light. The axis of the gleam has been seen at times, by practised observers, inclined at a considerable angle to the plane of the ecliptic. The extent of the zodiacal light

* While dealing with the relations presented by the Saturnian ring-system, in 1864, I was led to apply the formula, with suitable changes of elements, to the case of a ring circling the earth, being invited to the inquiry by the perusal of the observations made by Lieutenant Jones, and comments made thereon by Baron Humboldt. I found that there is not a single hypothesis as to the dimensions of such a ring which would lead to results according with, or even in the slightest degree approaching, the results of observations made upon the zodiacal light. This conclusion is embodied in a note at p. 117 of that treatise; in which note I remark that such investigations "prove that the zodiacal light cannot be due to a ring of a minute satellite surrounding the earth, the appearance of the ring in high latitudes being altogether different from that which would be presented by a ring surrounding the earth. I am careful to refer to these researches and their results, because remarks have been published implying that I have somewhat hastily come to a decision on the points here dealt with. A complete mathematical investigation of the subject made fully six years since may be regarded as fairly meeting those remarks.

has varied at times in the most remarkable manner, while its luminosity has been so variable that sometimes for months together it has been scarcely perceptible (in our northern latitudes); while at others it has been singularly conspicuous. I set on one side for the moment those observations by Lieutenant Jones which would imply that at times the zodiacal light increases so greatly in extent as to become visible at once both on the eastern and western horizon. I also set on one side those observations by M. Liais, according to which the zodiacal light can be seen at times extending as a complete arch from the eastern to the western horizon. Assuming these observations to be reliable (and those by M. Liais do not seem open to question), a true theory of the zodiacal light may be expected to account for them. But without insisting on this, it is evident, I think, that the admitted variations of the zodiacal light, in position, extent, and splendour, do not admit of being interpreted by the theory that the light is due to a disk including always the same materials moving in orbits of small eccentricity.

Nor do our difficulties seem removed if we assume that the constituents of the disk travel in orbits of considerable eccentricity, so long as we suppose that the actual constitution of the disk is constant, or nearly so, amidst whatever variations in the distribution of individual constituents.

Yet the general aspect of the zodiacal light, and the considerations already applied to other theories, suffice to prove that there is always present around the sun as centre a disk, whether composed of discrete meteorites, of vaporous masses, or of some combination of these and other forms of matter. The materials of this disk must be in motion around the sun in accordance with the laws of gravity; at least we have no evidence whatever inviting us to the supposition that they differ in this respect from all the other constituents of the solar system.

We are thus led to the conclusion that the bodies composing the zodiacal light travel in orbits of considerable eccentricity, carrying them far beyond the limits of what we may now term the zodiacal disk. The constitution of the disk thus becomes variable, and that within limits which may be exceedingly wide. They must be so, in fact, if all the recorded variations of the zodiacal light are to be accounted for. In other words, it is requisite (if our evidence is to be explained) not only that the paths of the materials composing the zodiacal light shall be for the most part very eccentric, but that along those paths the materials should not be strewn in such a way that a given portion of any path is at all times occupied by a constant, or nearly constant quantity of matter.

According to this view, the constituents of the zodiacal light

would (at least as respects distribution along their several paths and the general figure of those paths) resemble very closely the meteoric systems which, as we know, the earth traverses in the course of her annual motion around the sun.

By considering the zodiacal light we have thus been led to a theory involving, and associated with, the theory of meteor-systems as now established by the labours of Adams, Leverrier, Schiaparelli, and others. But it is worth noticing that, by reversing the process, and considering first the theory of meteor-systems so established, we are led quite as readily to the theory that there must at all times exist in the sun's neighbourhood a disk of discrete constituents which would present precisely such an appearance as the zodiacal light. I have shown elsewhere that this result is a simple mathematical deduction from the evidence.

But setting this consideration wholly on one side, the fact remains that all other theories of the zodiacal light (that is, of the motions of its constituent parts, without reference to its physical constitution) have been eliminated. It remains only to be shown that this theory is controverted by no peculiarities in the observed appearance of the zodiacal light, and also that we should inquire what further general laws, if any, may be predicated of the motions of the bodies composing this object.

The fact that the axis of the zodiacal light is ordinarily close to the ecliptic, is accounted for on the assumption that the various paths along which the constituents of the zodiacal disk travel, tend to aggregate towards the neighbourhood of the ecliptic. There is nothing, however, to prevent individual systems from having a considerable inclination to that plane.

The observed variation of the zodiacal light in brilliancy, position, and extent is obviously to be expected according to the view of its structure now under consideration.

The simultaneous appearance of an eastern and western light and Liais's observation of a complete arch of light have to be accounted for as highly exceptional but, at the same time, recognized phenomena. It is easy to see that both these phenomena may be regarded as indicating the occasional but very exceptional extension of the zodiacal disk to a considerable distance beyond the orbit of the earth. But it must not be concealed that there are great difficulties to be removed before this interpretation can be regarded as satisfactory.

Let us suppose, for instance, the case of a thin luminous disk occupying the whole orbit of Mars, and that the earth is in the part of her orbit where her distance north or south of this plane is greatest. Then it will be evident that the outline of the disk, as seen from any part of the earth, would correspond very nearly

to a great circle of the heavens, and that the whole of the visible heavens south or north of that great circle would be hidden by the luminous disk. In other words, a region of the heavens far larger than that occupied by the arch of *Liais*, or by the eastern and western lights of *Jones*, should be occupied by the zodiacal light if it had some such extension as we have assumed in the case of this luminous disk.

It is to be remembered, however, that, assuming (as we are bound to do) a considerable degree of flatness in the actual figure of the zodiacal disk, and more especially of its more distant portions, then much more light would be received from those parts towards which the line of sight is directed at a considerably acute angle, than from those parts which the line of sight crosses nearly at right angles. And it is easy to see that, on any reasonable assumption as to the range of zodiacal substance which it is necessary that the line of sight should traverse in order that any appreciable light may be received, the occasional visibility of the light where the superior planets alone can be seen becomes as readily explicable as the ordinary visibility of the light in those parts of the sky where the inferior planets become visible.

It will be seen that all that can be strictly said to have been demonstrated in this paper is the fact that the zodiacal light is associated with the sun, and not with the earth,—that it is not due to solar light reflected from bodies travelling within the earth's orbit, whether in circular or elliptic orbits,—and that, if the major part of the zodiacal light is reflected solar light, then the paths of the bodies reflecting that light must resemble those of the meteors encountered by the earth. As the spectroscope seems to show that at least a portion of the light* of the zodiacal gleam is not reflected solar light, we cannot, in the present state of our knowledge, definitely decide on a theory as to the motions of the bodies to which the light is due; for the solution of the problem is obviously bound up with the interpretation of the physical nature of the zodiacal light. If some solar action, for example, rouses luminosity in certain definite directions (as, for instance, near the plane of the sun's equator) in some such way as light is caused to appear along radial lines through and beyond the heads of comets, our power of theorizing from such considerations as have been dealt with in this paper would be

* I use this mode of speaking, not by any means as doubting the accuracy of Ångström's observation, but because, even if the greater part of the light gave a continuous spectrum, yet this spectrum might remain undiscernible even when bright lines corresponding to a very minute proportion of the total light were seen with ease. Nay, such bright lines as Ångström found in the spectrum of the phosphorescent light from the sky might be detected when a continuous spectrum from the much brighter light of the zodiacal radiance remained unseen.

limited. It would still remain certain that the zodiacal light is not a terrestrial appendage (either near or far off); but what sort of solar appendage it might be would be a problem as difficult to solve as that presented by comets.

If the radiated structure of the sun's corona as seen under favourable atmospheric conditions should be confirmed as more than an optical phenomenon, it is not impossible that we might be put in the way of interpreting the zodiacal light.

X. On the Hypothesis of Molecular Motions in Thermodynamics.

By W. J. MACQUORN RANKINE, C.E., LL.D., F.R.SS. Lond. & Edin.*

1. **I**F I rightly understand the question raised by Mr. Heath, it is not whether the two propositions called respectively the First Law and the Second Law of Thermodynamics correctly express the facts of experiments respecting the relations between heat and mechanical energy from which those laws have been arrived at by induction, but whether those two laws follow from the hypothesis that thermometric heat consists in some sort of molecular motion—and in particular whether the contraction of the space within which a system of moving bodies is confined causes of itself acceleration of their motion; for if this be established, the two laws of thermodynamics readily follow from the hypothesis.

2. According to a proposition laid down by all the writers on thermodynamics who have treated of the before-mentioned hypothesis, the acceleration of the motion of a system of bodies by the contraction of the space within which they are confined is a necessary consequence of the elementary principles of dynamics. According to Mr. Heath's views, unless I misunderstand them, that proposition is inconsistent with the principle that a body under the action of a pair of equal and opposite forces is neither accelerated nor retarded—in other words, with the first law of motion.

3. In order that the alleged inconsistency should exist, it would be necessary that each of the bodies under consideration should be under the action of no force, or of balanced forces. But that is not so; for the bodies under consideration are particles moving within a limited space; therefore their motions in any given direction are limited in extent; therefore during certain parts of their motions, and especially when they are near the boundaries of the containing space, those motions must be continually varying, certainly in direction, and possibly both in

* Communicated by the Author.

direction and in velocity ; therefore the particles cannot be under the action of balanced forces, nor of no force.

4. If one boundary of the limited space within which the moving particles are contained is a moveable piston, that piston may be at rest, or may have a uniform velocity, or a variable velocity ; and the condition to be fulfilled in order that the piston may be at rest, or may move uniformly, is, that the pressure applied to its outer surface shall be equal to the pressure exerted against its inner surface by the confined particles.

5. The various investigations of the consequences of the hypothesis of molecular motions with which I am acquainted, though differing much in detail, are all identical in principle ; and I do not know of any passage in the writings of any one of the authors of those investigations, which is, either avowedly or implicitly, in the slightest degree inconsistent with the dynamical principles which I now quote in Mr. Heath's words :—"No acceleration, and therefore no heat, is generated by the action of forces which are in equilibrium ;" and "where the force meets with no resistance . . . all the force is employed in producing acceleration."

6. With respect to the mode of applying those principles, the following remark may seem a truism, but is nevertheless of importance to the present discussion. In reasoning about the effects of forces, the body as to whose acceleration or retardation, or uniformity of motion, conclusions are drawn, ought to be the same with that to which the forces are applied ; and no conclusion as to the motion or rest of one body can be drawn from the fact of the equilibrium or non-equilibrium of the forces applied to another body. For example, if the outside and inside of a piston be acted on by equal and opposite pressures, the legitimate conclusion is that the piston will remain at rest or move at a uniform speed : the equilibrium of the forces applied to the piston proves nothing as to the state of motion or rest of the substance that is confined by it. If, on the other hand, the outside and inside of the piston be acted on by unequal pressures, the legitimate conclusion is that the piston will be accelerated or retarded, as the case may be ; and nothing can be concluded as to the state of the particles confined. In order to determine whether or to what extent these particles will be accelerated by an alteration of the space within which they are confined, we must consider whether there is any want of equilibrium amongst the forces exerted on them by the piston and by each other. Unless the particles are at rest, there cannot be equilibrium ; for they are not free to move uniformly in straight lines to an indefinite distance ; and their motions, if they move at all, must certainly be deflected, and possibly accelerated and retarded ; and such changes imply unbalanced forces.

7. As to my having in my paper of October "represented the piston as imparting *vis viva* to the particles without losing any of its own," I consider that I was warranted in doing so, because I provided, in the investigation, for the action of a pressure on the outside of the piston sufficient to balance that produced by the impulse of the particles against the inside, and therefore to impart to the piston exactly the same quantity of energy which it imparts to the particles.

It is easy, however, if desired, to provide for the possible retardation or acceleration of the piston through any inequality of the pressures exerted on its outer and inner faces, as follows.

Let u be the mean velocity of the piston during the small interval of time dt , and $-du$ the retardation during that interval. Let $-v$ be the normal component of the velocity, relatively to the vessel, with which a set of particles are moving towards the piston. The mean normal velocity of these particles, relatively to the piston, is $-(v+u)$; and the mean normal velocity, relatively to the piston, with which they rebound, is $+v+u$; and relatively to the vessel, $v+2u$.

Let mdt be the aggregate mass of the particles which act on the piston in the interval dt . Their mean total change of velocity is $2(v+u)$; therefore the outward pressure exerted by them on the piston, and the inward pressure exerted by the piston on them, are each equal to

$$Q = 2m(v+u). \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

The pressure exerted on the outside of the piston is

$$P = Q - \frac{Mdu}{dt}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

M being the mass of the piston; and as the piston moves inwards through the distance $u dt$, the work done by that pressure on the piston is

$$P u dt = Q u dt - M u du. \quad . \quad . \quad . \quad . \quad . \quad (3)$$

The energy lost by the piston through its retardation is $M u du$, which being added to the preceding quantity of work, gives for the work done by the piston on the confined particles,

$$P u dt + M u du = Q u dt = 2 m u (v+u) dt = 2 m (uv + u^2) dt. \quad . \quad (4)$$

The increase of energy of the mass mdt of moving particles is

$$\frac{mdt}{2} \{ (v+2u)^2 - v^2 \} = 2 m dt (uv + u^2), \quad . \quad . \quad (5)$$

being exactly equal to the sum of the work done by the external force on the piston, and the energy lost by the piston through

retardation. The case considered by me in my previous paper is that in which the external pressure is such that $\frac{du}{dt} = 0$.

The case in which there is no external pressure is expressed as follows:

$$Mu du = Q u dt = 2mu(v + u) dt; \quad . \quad . \quad . \quad . \quad (4A)$$

or, dividing both sides by $u dt$,

$$M \frac{du}{dt} = Q = 2m(v + u). \quad . \quad . \quad . \quad . \quad . \quad (4B)$$

8. With respect to the definition of "work," I may state that, so far as I know, that word is used by writers on dynamics to comprehend all sorts of effects that are produced by a force acting through a space, whether the overcoming of another force, such as an attraction, repulsion, or pressure, or the acceleration of a moving mass (see, for example, Thomson and Tait's 'Natural Philosophy,' § 241).

9. In the following investigation, the forces exerted by the confined particles on each other are taken into account. Let P be the pressure applied to the outside of the piston; Q the pressure exerted by the confined particles against the inside of the piston; let δx denote a distance through which the piston is or may be shifted, and be positive inwards; let M be the mass of the piston and u its velocity. Then, by the principle of the conservation of energy, we have

$$(P - Q)\delta x = Mu\delta u; \quad . \quad . \quad . \quad . \quad . \quad (6)$$

and in order that the velocity of the piston may be nothing or uniform (that is, in order that we may have either $u = 0$ or $\delta u = 0$), the condition to be fulfilled is $P - Q = 0$. The energy exerted by the external pressure in driving the piston inwards through the distance δx at a uniform velocity is $P\delta x$; the energy transferred from the piston to the particles on which it acts is $Q\delta x = P\delta x$; so that no energy is gained or lost by the piston.

10. Consider now the set of confined particles which are so near the piston as to exert upon it appreciable forces. The resultant or aggregate pressure exerted by that set of particles against the inner face of the piston is $-Q$; the resultant pressure exerted by the piston against the same set of particles is $+Q$. Let R (positive inwards, negative outwards) denote the resultant of all the attractions, repulsions, and pressures exerted on those particles by the other confined particles in directions parallel to x . Then $Q + R$ is the resultant of all the pressures, attractions, and repulsions exerted on the set of particles now under consideration, in directions parallel to x , whether by the piston or by the other particles. Let μ be the mass of that set of particles, and let Σ denote the summation of quantities relating

to every particle of the whole set. Then the dynamical equation of the set of particles, relatively to forces parallel to x , is as follows :

$$Q + R - \Sigma \mu \frac{d^2 x}{dt^2} = 0. \quad . \quad . \quad . \quad . \quad . \quad (7)$$

Let v be the total or resultant velocity of any one particle of the set, in whatsoever direction it may be moving ; then, when the piston is shifted through the distance δx , the following is the variation of the actual energy, or energy of motion of the particles :

$$\Sigma \mu v \delta v = (Q + R) \delta x = \Sigma \mu \frac{d^2 x}{dt^2} \delta x. \quad . \quad . \quad . \quad (8)$$

11. It is obvious, from what has been stated in § 5, that if the piston moves with a uniform velocity, we may substitute $P \delta x$ for $Q \delta x$; and the same is the case, indeed, if at the end of the motion denoted by δx the piston returns to the velocity which it had at the beginning of that motion, and also if the piston starts from a state of rest at the beginning and returns to a state of rest at the end of the motion δx . If, on the other hand, there is a change δu in the velocity of the piston during that motion, we must make

$$Q \delta x = P \delta x - M u \delta u, \quad . \quad . \quad . \quad . \quad (9)$$

in which u is to be taken to denote the mean between the velocities of the piston at the beginning and at the end of the motion δx ; and this equation may be used in the investigation of the motion of a bullet, driven by the pressure Q from within, and resisted by the pressure P from without.

12. The reasoning which has been applied to particles moving within a space of which a piston forms one boundary is evidently applicable to particles moving within any space capable of enlargement and contraction, and bounded by a surface of any kind which is not crossed by the moving particles. The boundary of the space may be a mathematical surface, having at its two sides systems of particles which meet and press or repel each other at the surface, but do not cross it ; and then the consideration of the mass of the piston may be omitted from the problem.

13. It can be shown that, for the particles at and near such a surface, the factor $\frac{d^2 x}{dt^2}$ in the expression for the energy gained by the confined particles through compression varies proportionally to v^2 , other circumstances being alike ; and hence may be deduced the second law of thermodynamics ; but this has so often been explained by different authors and according to different methods, that it is unnecessary to enlarge upon it now.

XI. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from vol. xl. p. 460.]

June 16, 1870.—General Sir Edward Sabine, K.C.B., President, in the Chair.

THE following communication was read :—

“On the Mathematical Theory of Combined Streams.” By W. J. Macquorn Rankine, C.E., LL.D., F.R.SS. Lond. and Edinb.

1. *Object of this Investigation.*—The principles of the action of combined streams were to a certain extent investigated by Venturi, and stated in his essay ‘*Sur la Communication latérale du Mouvement dans les Fluides*’ (Paris, 1798). The principle of the conservation of momentum, so far as I know, was first explicitly applied to combined streams by Mr. William Froude, F.R.S., in a paper on Giffard’s Injector, read to the British Association at Oxford, in 1860, and published in the Transactions of the Sections, p. 211. Various other authors have treated the same problem by different methods, based virtually on the same principle. A very complete and precise investigation of the theory of combined streams, in every case in which two streams only are combined, is contained in Professor Zeuner’s treatise ‘*Das Locomotivenblasrohr*’ (Zürich, 1863). The theoretical conclusions are tested by comparison with experiment, and applied to practical questions, especially those relating to the apparatus from which the treatise takes its name. The object of the present investigation is to apply similar principles to the combination of any number of streams; and the demonstration of the fundamental dynamic equation differs from that given by Zeuner in method, though not in principle, being effected at one operation by the direct application of the principle of the equality of impulse and momentum, instead of by the consideration of the loss of energy that takes place during the combination of the streams.

2. *Terms and Notation used, and Suppositions made.*—The several streams which are combined will be called before their junction, the *component streams*; the stream formed by their combination will be called the *resultant stream*. The passages through which the component and resultant streams flow will be called respectively the *supply-tubes* and the *discharge-tube*. The combination of the streams will be supposed to take place in a short cylindrical chamber, with its axis parallel to the direction of flow, which will be called the *junction-chamber*.

At one end of the junction-chamber are the outlets of the supply-tubes, which will be called the *nozzles*; at the other end, the inlet of the discharge-tube, which will be called the *throat*. It will be supposed, further, that the supply-tubes are so formed as to direct the component streams at the nozzles so that they shall all flow sensibly parallel to each other and to the resultant stream. The

principal symbols used are as follows. For any one of the component streams :—

a , area of nozzle ;

v , velocity of flow at nozzle ;

s_0 , bulkiness, or reciprocal of density, at nozzle.

The several component streams may be distinguished from each other, when required, by suffixes, as 1, 2, 3, &c.

For the resultant stream :—

A , area of throat ;

V , velocity of flow at throat ;

S_0 , bulkiness, or reciprocal of density, at throat.

Intensities of pressure, in *absolute units* on the unit of area :—

p_0 , at the nozzle end of junction-chamber ;

P_0 , at the throat.

(These may be converted into *units of weight* on the unit of area, by dividing by g .)

The flow of each stream is supposed to be steady. The fluids may be either liquid, vaporous, gaseous, or mixed.

3. *Equation of Continuity*.—The mass of fluid that enters the junction-chamber through a given nozzle in a unit of time is $\frac{av}{s_0}$.

The mass discharged in the same time at the throat is $\frac{AV}{S_0}$. The flow being steady, the following equation must at every instant be fulfilled :

$$\frac{AV}{S_0} = \Sigma \cdot \frac{av}{s_0} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

If S_0 and the several values of s_0 are given, that equation gives the velocity of the resultant stream in terms of those of the component streams ; viz.

$$V = \frac{S_0}{A} \cdot \Sigma \cdot \frac{av}{s_0} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1 A)$$

If all the fluids are liquids, each of sensibly invariable bulkiness, we have also $AV = \Sigma \cdot av$; that is, the volume of flow of the resultant stream is equal to the aggregate of the volumes of flow of the component streams ; but if any or all of the streams are vaporous or gaseous, the values of s_0 will depend upon that of p_0 , and the value of S_0 upon that of P_0 , and upon the changes of bulkiness of the fluids which may take place in the junction-chamber, through change of temperature, change of condition, or chemical action.

In any case S_0 may be regarded as a given function of P_0 , and of the mutual proportions of the several values of $\frac{av}{s_0}$; in other words, of the ingredients in the resultant stream.

4. *Dynamical Equation*.—The aggregate momentum of the mass of fluid that enters the junction-chamber through the nozzles in a unit of time is $\Sigma \cdot \frac{av^2}{s_0}$. The momentum of the equal mass which

leaves the junction-chamber through the throat in the same time is $\frac{AV^2}{S_0}$.

The forward impulse exerted in a unit of time upon the mass of fluid in the junction-chamber by the pressure at the nozzle end of the chamber is p_0A . The backward impulse exerted in the same time on the same mass by the pressure at the throat-end of the chamber is P_0A . By the second law of motion, the difference between those impulses is equal to the change of momentum produced; that is to say,

$$A(P_0 - p_0) = \Sigma \frac{av^2}{s_0} - \frac{AV^2}{S_0} = \Sigma \left\{ \frac{av}{s_0}(v - V) \right\}; \quad \dots \quad (2)$$

or, dividing both sides by A ,

$$P_0 - p_0 = \Sigma \frac{av^2}{As_0} - \frac{V^2}{S_0} = \Sigma \left\{ \frac{av}{As_0}(v - V) \right\}. \quad \dots \quad (2A)$$

And this is the general dynamical equation of the combination of any number of streams of any fluids.

If the preceding equation, as applied to a combination of two streams only, be compared with the equation not numbered, which immediately precedes equation 60 in Zeuner's treatise, it will be seen that they are virtually identical, although different in form, and demonstrated by different methods.

5. *Loss of Energy at Junction.*—If a given mass of any fluid at the bulkiness s and pressure p is contained in a reservoir, from which it is capable of being expelled by the inward motion of a piston loaded with an external force equivalent to the pressure, it is known that the potential energy of the mass of fluid and of the piston relatively to a point at the level of the centre of mass of the fluid is expressed by multiplying the mass by $\int_0^{p_0} s dp$, the relation between s and p being that which is called *adiabatic*; that is to say, such that no heat is received or given out by the fluid. Hence the loss of energy in the junction-chamber in each unit of time is given by the following expression:—

$$\Sigma \left\{ \frac{av}{s_0} \left(\frac{v^2}{2} + \int_0^{p_0} s dp \right) \right\} - \frac{AV}{S_0} \left(\frac{V^2}{2} + \int_0^{P_0} S dP \right), \quad \dots \quad (3)$$

of which the first, or positive term, denotes the aggregate energy, actual and potential, of the component streams as they enter the junction-chamber; and the second, or negative term, expresses the total energy, actual and potential, of the resultant stream as it leaves that chamber. That lost energy takes the form partly of visible eddies and partly of invisible molecular motions—that is, of heat.

The integral expressing the aggregate potential energy of the component streams may be put in the following form:—

$$\int_0^{p_0} \left(\Sigma \frac{avs}{s_0} \right) dp. \quad \dots \quad (3A)$$

If no change of total bulkiness arises from the mixture of the component streams, the volume occupied by a given mass of the mixture is simply the sum of the volumes of its ingredients; so that we have

$$\frac{AVS}{S_0} = \sum \frac{avs}{s_0}, \quad \dots \dots \dots (3B)$$

and the expression for the loss of energy becomes

$$\sum \cdot \frac{av^3}{2s_0} - \frac{AV^3}{2S_0} - \frac{AV}{S_0} \int_{p_0}^{P_0} S dP. \quad \dots \dots \dots (3C)$$

When the fluids are all liquids, whose compressibility may be neglected, we have $\int_{p_0}^{P_0} S dP = S_0 (P_0 - p_0)$; and substituting for the difference of pressures its value according to equation (2), the following expression is found for the loss of energy at the junction,

$$\sum \cdot \left\{ \frac{av}{s_0} \cdot \frac{(v-V)^2}{2} \right\}; \quad \dots \dots \dots (3D)$$

that is to say, in the case of liquids all the energy due to the several velocities $(v-V)$ of the component streams relatively to the *resultant stream* is lost.

When the expression (3D) is reduced to a single term, it becomes the well-known value of the loss of energy of a single stream of liquid at a sudden enlargement in a tube.

6. *Efficiency of Combined Streams.*—The *efficiency* of a set of combined streams may be defined as the fraction expressing the ratio borne by the total energy of the resultant stream after the combination to the aggregate energy of the component streams before the combination. It is expressed as follows:—

$$\frac{\frac{AV}{S_0} \left\{ \frac{V^2}{2} + \int_0^{P_0} S dP \right\}}{\sum \left\{ \frac{av}{s_0} \left(\frac{v^2}{2} + \int_0^{p_0} s dp \right) \right\}} \cdot \dots \dots \dots (4)$$

7. *General Problem of Combined Streams.*—In most cases the problem of combined streams takes one or the other of the two following forms. In each of the two forms the areas of the nozzles a_1, a_2 , &c. are given, and also the area of the throat, A .

First Form.—The quantities given, besides the before-mentioned areas, are the pressure at the nozzles, p_0 , and the velocities of the component streams, v_1 , &c. The functional values given are those of

$s_{0,1}, s_{0,2}$, &c., in terms of p_0 , and of S_0 in terms of $P_0, \frac{a_1 v_1}{s_{0,1}}, \frac{a_2 v_2}{s_{0,2}}$,

&c. Those functional values are to be substituted in the equations (1) and (2); and the solution of these equations will give the numerical values of V and of P_0 . In the case of liquids of sensibly constant bulkiness, $s_{0,1}$ &c., and S_0 are quantities sensibly independent

of p_0 and P_0 ; and then equations (1) and (2) can be separately solved without elimination, giving respectively V and P_0 .

Second Form.—Each of the component streams flows through a passage whose factor of resistance, f , is given, from a separate reservoir in which the pressure p and the elevation z of the surface above the junction-chamber are given. The resultant stream flows through a passage whose factor of resistance, F , is given, into a reservoir in which the pressure P and the elevation Z of the surface above the junction-chamber are given. These, together with the areas A , a_1 , a_2 , &c., are the quantities given. The functional values given are those of the bulkiness, $s_{0,1}$, $s_{0,2}$, &c. and S_0 , as before; also the following values of the velocities, according to well-known principles in hydrodynamics. For any component stream,

$$v = \sqrt{\left\{ \frac{2gz + 2 \int_{p_0}^p s dp}{1 + f} \right\}}; \quad (5)$$

and for the resultant stream,

$$V = \sqrt{\left\{ \frac{2gZ + 2 \int_{P_0}^P S dP}{1 + F} \right\}}; \quad (6)$$

The functional values given are to be substituted in equations (1) and (2), whose solution will then give the numerical values of p_0 and P_0 ; and from these and the other data the numerical values of v_1 &c. and of V may be calculated.

GEOLOGICAL SOCIETY.

[Continued from vol. xl. p. 388.]

June 8th, 1870.—Joseph Prestwich, Esq., F.R.S., President,
in the Chair.

The following communications were read:—

1. "On the Superficial Deposits of the South of Hampshire and the Isle of Wight." By Thomas Codrington, Esq., F.G.S.

This paper treated of the gravel deposits covering the tertiary strata of the country between Portsmouth and Poole, and of the Isle of Wight.

The strikingly tabular character of the surface is best seen on the east of the Avon, where from the coast for more than twenty miles inland a gravel-covered plain can be followed, rising gradually from 80 feet to 420 feet above the sea, at the rate of about 20 feet per mile. The high plains of the New Forest, to the eye perfectly level, and indented by deep valleys, are portions of this table-land. The plateau between the Bournemouth Cliffs and the Valley of the Stour, and detached gravel-capped hills further inland, are the remnants of a similar table-land on the west of the Avon, while eastwards the same character prevails up to Southampton Water. Sections parallel with the coast show the level nature of the country, broken only by well-defined river-valleys. On the east of Southampton Water a

similar tabular surface, sloping at a steeper angle towards the shore-line, and cut through by the valleys of the Itchen, Hamble, and Titchfield rivers remains; and in the Isle of Wight the gravels capping the flat-topped tertiary hills coincide with a corresponding plain sloping northwards.

The gravel covering these table-lands is composed chiefly of sub-angular chalk-flints, with a varying proportion of tertiary pebbles. Sarsen stone blocks are found everywhere; and on Poole Heath granitic pebbles, and in the gravel of Portsea large boulders of granitic and palæozoic rocks are met with. In the Isle of Wight, chert from the Upper Greensand and materials from the Lower Cretaceous beds also occur. The colour of the gravel is generally red; and the origin of the white gravel, which often overlies the red, is to be ascribed to the bleaching action of vegetable matter. Brick-earth is generally associated with the gravel at all levels but the highest; but the contorted appearances attributed to glacial action only occur at low levels.

No organic remains have been found in the gravel covering the plains, while the valley-gravels of the district have afforded mammalian bones and teeth of the usual species. Flint implements have been found at Bournemouth at 120 feet above the sea, at Ly-mington, near Southampton, at 80 and 150 feet, and also along the shore between Southampton Water and Gosport, at 35 feet above the sea, from gravel forming part of the covering of the tabular surface, and unconnected with the river-valleys.

The gravel capping the cliffs of the south coast of the Isle of Wight, in which the remains of *Elephas primigenius* have been found near Brook and Grange, was probably deposited in the same river-basin as the mammaliferous gravel of Freshwater; and the cutting back of the coast-line by the sea has given the tributaries of a river which flowed by Freshwater northwards to the Solent a direct outfall to the sea; and the streams thus intercepted at a high level under the changed condition of flow, have originated the *Chînes*.

The gravel cliff of the Foreland, at the eastern end of the Isle of Wight, consists principally of raised shingle, which towards the south thins out, and is overlain by a thick deposit of brick-earth, a continuation of which caps the cliffs up to the chalk, and in which a flint implement was found by the author at 85 feet above the sea.

General Considerations.—The marine gravel, with granite boulders covering the south of Sussex, is continued westward by the gravel with similar boulders covering Portsea Island; and this again by the Hill-head gravels, with large blocks of Sarsen stone, these lower gravels being bordered on the south by the raised shingle deposits of the Isle of Wight, and on the north by the higher marine gravels of Avisford, Waterbeach, and Bourne, from which the lower gravel is divided by a well-marked step, extending beyond Portsdown Hill to Titchfield, and traceable on the west of Southampton Water. The Hill-head gravels are considered to be an estuarine deposit, of the same age as the marine gravels of Sussex

and the low-level gravels of the river-valleys; they are supposed to have been formed when the Isle of Wight was still joined to the mainland, and all the rivers now reaching the sea by Poole Harbour, Christchurch Harbour, Southampton Water, &c. were affluents of a river communicating with an estuary opening to the sea in the direction of Spithead.

The gravels lying above the step, such as those of Avisford and Waterbeach, Titchfield Common, Beaulieu Heath, and Bournemouth, are looked upon as equivalent in position and age to the high-level valley gravels.

The level of the gravels on the highest parts of the table-lands is such as to indicate an age far greater than that of the highest gravels of the river-valleys; but the uniform surface from the 400-foot level downwards points to a long continuance of similar conditions, during which the gravel from the highest levels to that of the Bournemouth cliffs was deposited. The area that can with any probability be assigned to the catchment-basin of a river such as that which has been before alluded to, is only three quarters of the basin of the Thames above Hampton, within which it is difficult to imagine that such an extent of gravel could have been spread out; and the inclination of the flattest of the table-lands is for a river such as only mountain-streams have, and quite incompatible with the spreading out of large even surfaces more than twenty miles across.

It is considered more probable that the materials of the gravel were brought down from the chalk country on all sides by rivers, and spread out in an inlet of the sea shut in on the south, and opening out eastwards. This view is not without difficulties: it involves a gradual upheaval of the land, which, when the highest gravels now remaining were being spread out at or near the sea-level, must have stood more than 400 feet lower; and a considerable part of this upheaval must have taken place since the formation of the gravel in which implements fashioned by man are imbedded.

2. "On the relative position of the Forest-bed and the Chillesford Clay in Norfolk and Suffolk, and on the real position of the Forest-bed." By the Rev. John Gunn, M.A., F.G.S.

The author commenced by stating that both at Easton Bavent and at Kessingland the Forest-bed is to be seen forming part of the beach, or of the foot of the cliff, and underlying the Chillesford Clay. He considered that the soil of the Forest-bed had been deposited in an estuary, and that after its elevation the trees, of which the stools are now visible along the coast, grew upon it, and the true Forest-bed was formed. After the submergence of this, first freshwater, then fluvio-marine, and finally marine deposits were formed upon it; and the author proposed to give the whole of these deposits the name of the "Forest-bed series." The author suggested that the Forest-bed itself is represented inland by the stony bed which lies immediately upon the chalk and between it and the Fluvio-marine and Marine Crag, his theory being that the surface of the Chalk, after supporting a Forest-bed fauna, was gradually covered up by successive Crag deposits.

3. "On a new Labyrinthodont Amphibian from the **Magnesian Limestone of Midderidge, Durham.**" By Albany Hancock, Esq., F.L.S., and Richard Howse, Esq.

The specimen described by the authors was remarkable for the great development of the ventral scutes, which were of large size, minutely striated, imbricated and arranged in diagonal lines, sloping from behind forward, giving the surface of the fossil a strongly ribbed appearance. The authors stated that it was allied to *Dasyceps* and to *Pholiderpeton scutigerum* (Huxl.); but, from the peculiar characters of its scutes, its elongated narrow muzzle, and some other peculiarities presented by the head, they regarded it as forming the type of a new genus, which they proposed to name *Lepidotosaurus*. The species they named *L. Duffii*, in honour of its discoverer, Mr. Joseph Duff.

4. "On *Proterosaurus Speneri*, von Meyer, and a new species, *Proterosaurus Huxleyi*, from the Marl-slate of Midderidge, Durham." By Albany Hancock, Esq., F.L.S., and Richard Howse, Esq.

In this paper the authors described a specimen which they referred to *Proterosaurus Speneri*, von Meyer—and one of a smaller form, which they regarded as new, and described as *Proterosaurus Huxleyi*. Both were from the same part of the Marl-slate of Midderidge, Durham. The two species agree in having the limbs and tail long, and the neck long and composed of seven vertebræ, in the number of dorsal vertebræ, in the number and character of the bones of the hand, and in some other particulars, sufficient, with these, in the opinion of the authors, to justify the reference of both to the genus *Proterosaurus*. In *P. Huxleyi* the ribs are flattened instead of rounded at the proximal extremity, and less widened and grooved at the distal extremity than in *P. Speneri*; the hind limb is considerably longer in proportion to the fore limb; and the distal extremity of the humerus is only twice as wide as the constricted part, instead of three times, as in the old species.

June 22, 1870.—Joseph Prestwich, Esq., F.R.S., President,
in the Chair.

The following communications were read:—

1. "Notes on the Lower portion of the Green-slates and Porphyries of the Lake District between Ulleswater and Keswick." By H. Alleyne Nicholson, M.D., D.Sc., M.A., F.R.S.E., F.G.S., Lecturer on Natural History in the Medical School of Edinburgh.

The author describes the characters presented by the lower part of that series of rocks, named by Professor Sedgwick the "Green-slates and Porphyries," which overlie the Skiddaw Slates in the Lake District. He notices the sections of this series in Borrowdale, on the east side of Derwentwater, between Keswick and the Vale of St. John, in the Vale of St. John, in Matterdale, in Eycott Hill, between Ulleswater and Haweswater, and in the neighbourhood of Shap. In the Borrowdale section the sequence of the rocks is given by the author as follows. Resting on the Skiddaw slates there are:—(1) a felspathic trap; (2) a great series of ashes, breccias, and amyg-

daloids, often showing slaty cleavage and worked as slates, but with several intercalated bands of trap; and (3) a second trap. This appears to be a normal section, and is repeated, but diversified by the results of folding and faults, in the other localities described by the author, except that in the Vale of St. John the true slaty series seems to be entirely wanting.

2. "Observations on some Vegetable Fossils from Victoria." By Dr. Ferdinand von Müller and R. Brough Smyth, Esq., F.G.S.

Mr. Smyth stated that the fossils, of which specimens were forwarded by him, were obtained in one of the deep leads at Haddon, near Smythesdale. No leaves have been obtained from the bed, which consists of a greyish-black clay; the fruits and seed-vessels were obtained about 180 feet from the surface, and represent a flora not very dissimilar to that now characterizing some parts of Queensland. The specimens sent include the fruits of a supposed new genus of Coniferæ, described by Dr. von Müller under the name of *Spondylostrobos*. It is most nearly allied to *Solenostrobos*, Bowerbank; but its five valves are not keeled. The columella forms the main body of the fruit; and the seeds are apparently solitary. The species was named *Spondylostrobos Smythii*. The remaining specimens consisted of:—a solitary fruit of a genus of Verbenaceæ; an indehiscent compressed fruit, probably belonging to the proteaceous genus *Helicia*; a nut nearly allied to the preceding; a large, spherical, unilocular, 3-seeded nut with a thick pericarp, perhaps from a Capparideous plant; a 5-valved capsule of an unknown genus; and fruit-valves of three other plants, probably belonging to the Sapindaceæ, and perhaps allied to *Cupania*. One of the last may belong to the Meliaceous genus *Dysoxylon*. Dr. Müller considered that these remains indicate a former flora analogous to that of the existing forest-belt of Eastern Australia.

3. "Note on some Plesiosaurian Remains obtained by J. C. Mansel, Esq., F.G.S., in Kimmeridge Bay, Dorset. By J. W. Hulke, Esq., F.R.S., F.G.S.

The remains described in this Note represent two new species of *Plesiosaurus*. The dorsal vertebræ of the first species are distinguished by extremely short centra, with hollow articular faces. The antero-posterior diameter of 4 centra ranges between 1 and 1·3 inch, the transverse horizontal diameter between 4 and 4·6, and the vertical between 3·8 and 4 inches. For this Plesiosaur the author proposes the specific name of *P. brachistospondylus*.

The other species, of which the greater part of the spinal column and portions of the breast and pelvic girdles and limbs are preserved, is a long slender-necked Plesiosaur exceeding 16 feet in length. Its limbs are much larger in proportion to the whole length than in the typical Liassic forms of this genus; but what particularly distinguishes it from these are the massiveness of the humerus and femur, the larger size of the wing-like expansion of the postaxial border, a well-developed trochanter, and especially three articular

facets at the distal end of the femur, corresponding to which the second segment of the paddle, representing the leg, contains three coequal bones. The author noticed the impression of a third bone in this segment in the matrix in which a paddle of *Pliosaurus portlandicus* is imbedded, and the ossicle on the postaxial border of the fibula in *Plesiosaurus rugosus*. He compared the paddle-bones of the Kimmeridge *Plesiosaurus* with those of *Ichthyosauri* and of the Liassic *Plesiosaurs* and of *Pliosaurus*; he drew attention to the very close resemblance of the humerus and femur to type specimens of the femora of *Pliosaurus brachyleirus* and *P. trochanterius* in the British Museum, and traced a similar resemblance between the elements of the enemion and tarsus and those of the Dorchester and Portland Pliosaurian paddles. For this creature, combining a long, truly Plesiosaurian neck with Pliosaurian-like limbs, the author proposed the name of *Plesiosaurus Manselii*.

4. "Notes on the Geology of the Lofoten Islands." By T. G. Bonney, M.A., F.G.S., Tutor of St. John's College, Cambridge.

The author described the general appearance of the Lofoten Islands, which have commonly been described as composed of granite, but which, he stated, really consist of gneissic rocks. The scenery of some of the islands, on which he did not land, resembled that of the Cambrian and Cambro-Silurian districts of Wales and Cumberland; and the interior of Hassel showed dark rounded fells, resembling in outline some of the softer Welsh slates. At Stokmarknæs and at Melbø there is a granitoid rock of pinkish-grey colour, consisting of felspar and platy hornblende, with some mica and quartz. The Svolvaer Fjeld in Ost Vaagø shows a distinctly bedded structure in the cliffs near Svolvaer, the débris at the foot of which consist of a rock resembling syenite, and a quartzite containing a little hornblende and felspar. Bedding was also observed towards the Oxnæs Fjord. The islets near this coast consisted chiefly of a granitoid rock resembling a syenite, showing traces of bedding to the west of Svolvaer. Seams and veins of quartz, hornblende, &c. occurred in some of the islets; and these were sometimes too regular to be explained by deposition in fissures. Near the Svolvaer post-office there was gneiss coarsely foliated, containing hornblende and mica, with pink orthoclase felspar. The author concluded, from his observations, that, with few exceptions, the so-called granites of the Lofoten Islands are stratified, highly metamorphosed rocks, quartzites, and gneiss, generally with much felspar in the latter, and with more or less hornblende in both, and that they are inferior in position to the gneiss and schists of the mainland, and to the more slaty rocks of the southern and western parts of the same islands. He compares them with some gneiss from Dalbeg on the west coast of the island of Lewis.

5. "On *Dorypterus Hofmanni*, Germar, from the Marl-slate of Midderidge, Durham." By Albany Hancock, Esq., F.L.S., and Richard Howse, Esq.

The material for this paper consisted of four specimens of *Do-*

rypterus Hofmanni, which have been discovered by Joseph Duff, Esq., in the marl-slate of Midderidge, and are believed to be the first examples of this Fish which have been obtained in this country. The stratum from which they were procured is the same as that described by Prof. Sedgwick in his paper published in the Transactions of this Society (2nd series, vol. iii. pp. 76-77). The specimens show that the "ribbon-shaped" process mentioned by Germar is part of a peculiar exoskeleton, and that *Dorypterus* possessed ventral fins, which were situated in front of the pectorals, or "jugular." Hitherto no fishes with ventral fins other than "abdominal" in position have been known to occur earlier than the Cretaceous epoch. The tail is heterocercal, not homocercal as Germar supposed. The dentition is not displayed in any of the specimens, and the teeth were probably small and inconspicuous; but the general structure of the fish shows it to be most nearly allied to the Pycnodonts.

6. "Observations on Ice-marks in Newfoundland." By Staff Commander J. H. Kerr, R.N., F.R.G.S.

The author describes and tabulates the grooves and scratches observed by him on rock-surfaces in various parts of Newfoundland, especially Conception Bay, the neighbourhood of St. John's, and the north of Bonavista Bay. From the diversity of the direction of the markings and other considerations, he considers that they must have been produced by glaciers, and he believes that the main features of the country were much the same as at present before the glaciation took place. The author thinks that the land has not been submerged since it was freed from its coating of ice.

7. "On the Glacial Phenomena of Western Lancashire and Cheshire." By C. E. De Rance, Esq., F.G.S.

The author described the general form of the ground and the pre-glacial condition and glacial deposits of the districts of Wirral and Western Lancashire, and draws from his observations the following general conclusions. That before and at the commencement of the glacial epoch the north-west of England was more elevated above the sea-level than at present, but afterwards gradually subsided, during which process marine denudation produced the plains of Wirral and Western Lancashire. Part of the latter has since been covered with glacial deposits 200 feet thick. The valleys running in the strike of the Triassic strata appear to have been formed by subaërial agencies. It is probable that when the glacial epoch commenced the hilly country was covered with immense glaciers, or with an ice-sheet, which, as the land sunk, reached the sea. *The High-level Lower Boulder-clay* was probably produced by this land-ice. The land continued subsiding until it stood 100 feet lower than at present, submerging the lowlands of Lancashire and Cheshire to a depth of rather less than 25 fathoms, the coast-line being surrounded by an ice-foot, which received on its surface quantities of pebbles and boulders from the lake-district. These, on the

breaking up of the ice-foot, were spread over the lowlands, forming the *Low-level Lower Boulder-clay*. The climate then improved, although subsidence still continued, and the sandy and gravelly deposits of the middle drift were produced—these deposits, at whatever elevation they occur, having been found in shallow water during the constant subsidence of the coast-line. The surface of the middle Drift shows traces of what seems to have been subaërial erosion, leading to the supposition that the land must have risen and suffered denudation before that depression during which the Upper Boulder-clay was deposited, at which period the climate again became extremely cold, and fresh glaciers were formed. Before the elevation of the Upper Boulder-clay the climate was greatly ameliorated.

8. "On the Preglacial Deposits of Western Lancashire and Cheshire." By C. E. De Rance, Esq., F.G.S.

The author believed that after the deposition of the Esker Drift the country rose to from 200 to 300 feet higher than at present; but in the course of this elevation there was a pause, during which denudation took place, and the low plains, now covered with peat-moss, came into existence. From the consideration of the present depths of the channel between Great Britain and Ireland, the author inferred that an elevation of 200 feet would have caused the coast-line to run from the Mull of Galloway to St. David's Head; and Ireland would have been so connected with Wales as to render possible the migration of mammals, plants, and of man himself. Glaciers probably still persisted in the lake-district during the whole of this period of elevation. During a subsequent subsidence drainage became greatly obstructed, peat was formed, the sea encroached upon the land and worked its way eastward over the seabottom of postglacial times, a movement yet in progress. Here and there sand has begun to blow, forming dunes.

9. "Observations on Modern Glacial Action in Canada." By the Rev. W. Bleasdel, M.A., Rector of Trenton.

The author described some phenomena of ice-transport observed in Canada, especially those produced by the flood, anchor, or pack-ice produced in the rapids of the Canadian rivers. To this he attributed the entire disappearance of Crab Island in the River St. Lawrence, near Cornwall. This island occupied about an acre and a half within the memory of men now living; it has now entirely disappeared, and the water above it is gradually deepening. The island, according to the author, has been carried away piecemeal by the action of miniature icebergs, floated off by a rise in the water produced by a dam of anchor-ice below.

XII. *Intelligence and Miscellaneous Articles.*

ON THE SPECTRUM OF THE AURORA BOREALIS.

BY JOHN BROWNING, ESQ.

DURING the display of the Aurora Borealis which occurred on the evenings of the 24th and 25th of October, I confined my attention to observing the spectra of the light, taking it in different parts of the sky. When the spectroscope was directed to the more luminous portions, which were generally of a silvery white, the spectrum appeared to me to consist of only one line. I could not succeed in verifying the position of this line; but it appeared to be situated between D and E in the spectrum. When observing the light of the red portions of the sky, a faint red line became visible. I had no means of verifying the position of these lines with any degree of exactitude; but I was able to throw into the field of view a faint continuous spectrum from a distant light, and also the bright yellow sodium-line produced by a spirit-lamp.

The colour of the green line was very peculiar; had I not been able to observe it by comparison, I could not have formed any idea of its position. It was an exceedingly light silvery green, or greenish grey, and often seemed to flicker. Besides the two lines I have particularly described, I occasionally suspected others, one in the red and one in the blue; but I could not be at all sure of this. The colour of the light of the aurora seen over the greater portion of the heavens resembled exactly that of the discharge of electricity from an induction-coil through a vacuum formed from atmospheric air.—*Monthly Notices of the Royal Astronomical Society*, November 11, 1870.

NOTE ON THE USE OF EYE-SCREENS IN TELESCOPIC AND OTHER RESEARCHES. BY RICHARD A. PROCTOR, B.A.

Most observers with the telescope keep the unemployed eye open while studying with the other the features of an object in the telescopic field of view. Closing the unemployed eye produces, in fact, palpably injurious effects. But I suppose every one must have noticed how much better the features of a faint object are seen, when with the hand or any screen the unemployed eye is protected, even though it be only from the luminosity of the star-lit sky. I find that much better results are obtained when the unemployed eye is guarded from the access of all light whatever by a screen lined with black velvet. Yet better vision is secured if the screen is so contrived as to guard the employed eye also from all light except what reaches it from the object. This can easily be managed by having a screen covering both eyes, but with sliding doors, by which a small circular aperture opposite either eye can be opened or shut at will. Each aperture can have outside it a small tube passing over the eye-tube of the telescope. Or (preferably for some purposes) a small sleeve-curtain with an elastic circular opening at one end can be attached at the other over either eye-aperture. Then if

the opening be passed over the eye-tube and suffered to close by its elasticity, the desired result will be secured.

From my own experience I feel confident that, for certain special orders of research, the use of eye-screens of this sort (the details being arranged according to convenience) will be found of great service. I may particularize observation on moonlit nights, the examination of faint nebulae or the search for them, the search for minute points of light, delicate spectroscopic and polariscopic researches, and the investigation of questions concerning the colours of the planets, of the lunar regions, &c. The following description of the *modus operandi* will serve to supplement the above general sketch. The equatorial being set on any delicate object, the observer fastens the screen over his eyes, attaches the sleeve-curtain over the eye he proposes to work with, slides the elastic ring over the eye-tube, and suffers it to close round that tube. Having seen that the object is in the field and the rate of driving just, he closes both the eye-doors, keeping his eyes open and directed on the perfect blackness of the velvet lining. When he thinks the eye sufficiently prepared, he draws out the sliding eye-door over the telescope-eye, and is then able to apply the full powers of his eyesight (under exceptionally favourable conditions) to the examination of the object he has to deal with,

I am desirous of learning how far the efficiency of such contrivances depends on aperture—that is, whether the performance of large telescopes is as much improved as that of smaller instruments. If any Fellows of the Society should care to experiment on this matter (which is not unimportant, I conceive), they would be conferring a favour on me by communicating the results they may obtain. I refer here simply to the comparison between the work of a telescope used in the ordinary way and when supplemented by eye-screens.

I may renew here my suggestion of the great importance of applying such contrivances during the examination of the corona in total solar eclipses, whether with the telescope, spectroscope, or polariscope. As regards the spectroscopic analysis of the corona, any method by which the visibility of a very faint continuous spectrum can be increased (and I imagine that eye-screens such as I have proposed would have such an effect) cannot but be well worth trying. It seems highly probable that the variations in the accounts given so far depend wholly on the amount of opening in the slit—a narrow slit giving a very faint continuous spectrum and three somewhat brighter lines (one can scarcely speak of bright lines in this case), which are under these circumstances very difficult and delicate objects of observation; a somewhat wider opening gives a continuous spectrum, bright enough to obliterate two of the lines, but leaving one (the one Professor Harkness saw) fairly visible; a wider opening shows only a continuous spectrum, the lines being all obliterated. This at least is the explanation which seems alone consistent with the principles of spectroscopic analysis; and it is obvious that, if it be just, any increase in the eye's power of appreciating faint light must be a great gain.—*Monthly Notices of the Royal Astronomical Society*, November 11, 1870.

THE
LONDON, EDINBURGH, AND DUBLIN
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

[FOURTH SERIES.]

FEBRUARY 1871.

XIII. *On the Cause of the Motion of Glaciers.*

By JOHN BALL, *F.R.S.**

HAVING been absent from England, I have but lately seen the interesting paper by Mr. James Croll in the last volume of this Journal, wherein, continuing the discussion respecting the theory of glacier-motion put forth by the Rev. H. Moseley, he has done me the honour of replying to the remarks made by me on the same subject in July last, and at the same time has developed rather more fully the views previously published by himself, which may for brevity be designated the *molecular theory* of glacier-motion.

I must, in the first place, confess that, in the few pages published in this Journal for last July, I gave a very incomplete statement of my own opinions in regard to the cause of the motion of glaciers. My object was to state very briefly what seemed to me insurmountable objections to the arguments used by Mr. Moseley; and even in this respect I fear that I have failed to express myself with the requisite fulness and precision. I now beg permission partially to supply the deficiency, and at the same time to explain why I think it unnecessary to supplement the received theory of glacier-motion by a hypothetical assumption as to the molecular condition of the ice in a moving glacier, and to what extent I think that theory does require modification.

I am happy to find myself in full agreement with Mr. Croll respecting the so-called "crawling" theory of Mr. Moseley, of which no defender has been found amongst men familiar with the

* Communicated by the Author.

facts of glacier-motion. The point for discussion is whether the ordinary theory, which affirms that a glacier descends by its weight through the processes of fracture and regelation, has been overthrown by the arguments and observations opposed to it by Mr. Moseley. Mr. Croll admits the force of some of the objections urged by me to the views of Canon Moseley, but nevertheless retains the opinion already expressed by him in March 1869, that "Canon Moseley has successfully shown the insufficiency of the generally received theory of the descent of glaciers."

It is known to all who have followed the controversy, that the argument against the received theory depends upon observations made by Mr. Moseley on the *shearing-force* of ice, determined by the weight or pressure requisite to shear a square inch of that substance. In my last paper I sought indirectly to show that these observations were inapplicable, because the physical fact which Mr. Moseley had in view is not the same, but totally different from that which obtains in a moving glacier. It will perhaps be simpler to say that I demur altogether to the use of the term *shearing-force* until its meaning is accurately defined, and is shown to be appropriate to the argument to which it is applied. In some substances the amount of resistance opposed to the separation of adjoining particles is nearly independent of temperature, and of the time during which the pressure is applied. In other bodies which oppose a very considerable resistance to fracture, the particles gradually change their relative positions under the prolonged action of even slight pressure; and in some of them the amount of change depends very much on their temperature while exposed to pressure. If we are to use the same term for every process by which the particles of solid bodies change their relative positions, whether by actual fracture or by more or less slow rearrangement, we shall merely introduce confusion into our physical conceptions. What, to take a familiar instance, is the unit of shear of ordinary sealing-wax? Measured by the method employed by Mr. Moseley, it would be represented by a considerable weight, which would not vary very much within the ordinary range of temperature in our rooms. His argument would apply at least as well to show that a glacier of sealing-wax at, say, 20° below its point of fusion could not descend from the mountains into the valleys, though it most certainly would do so far more rapidly than our ice-glaciers do.

I must be allowed to remind Mr. Croll that the utmost amount of relative displacement of the particles of ice in a moving glacier amounts to a difference of the sixteenth part of a line in twenty-four hours between the motion of two points one inch apart. If any one will make careful observations to ascertain the pressure necessary to produce that amount of dis-

placement in glacier-ice, not in compact ice from our ponds, he will have ascertained a fact that is at least *prima facie* applicable to the consideration of the cause of glacier-motion; but as in the present state of our knowledge the observations of Mr. Moseley do not contribute in the slightest degree to determine the amount of that pressure, I must continue to deny the cogency of the argument derived from them. I shall presently endeavour to show that the determination of the unknown force in question would by no means conclude the controversy, even though the resistance were much greater than the force of gravity acting on a given section of the glacier-mass; but I wish to make a further preliminary remark on this portion of the subject.

Mr. Croll explicitly admits that Mr. Moseley's argument is defective, inasmuch as he takes no account of the time during which a given pressure must be applied in order to shear adjoining surfaces of ice; and, in accordance with the views which he holds as to the molecular action of heat upon ice, he assumes that the reason why time enters as a condition into the phenomenon is that it is necessary for the communication of heat, and enters as a condition merely so far as it determines the quantity of heat received. Reasoning on this basis, Mr. Croll argues that, inasmuch as in Canon Moseley's experiments the results obtained were due to pressure *plus* a certain small amount of heat, if they had been obtained with less pressure acting for a longer time, they would have been the result of a greater amount of heat combined with the pressure. The unit of shear determined by Canon Moseley is certainly not too large. Having concluded from these premises that the existing theory has been overthrown by Canon Moseley's objections, Mr. Croll introduces his own very ingenious views as to the molecular effects of the propagation of heat through ice as affording the only possible explanation of the phenomena of glacier-motion.

With the most sincere respect for Mr. Croll's acuteness and the usual accuracy of his reasoning, I think that on this occasion there is an obvious begging of the question, in first assuming that his molecular hypothesis is alone capable of accounting for the fact that the effect of pressure upon some bodies largely depends upon the time during which it acts, and then arguing from this fact to show that it is necessary to admit the truth of the hypothesis. There is a large class of bodies which are capable of changing their molecular condition and becoming fluid by the application of heat, but which are also capable of modifying their form, or undergoing changes in the relative positions of their particles at temperatures very much below their melting-point. Is Mr. Croll prepared to maintain that in

every such instance the motion is to be accounted for only by supposing conversion into the fluid form of the particles at the moment when they are changing their relative positions? Or if not, why should he imagine that a special hypothesis is necessary in regard to ice? So far as I know, the fact that ice will change its form under moderate pressure without fracture at a temperature notably below its melting-point was, for the first time, established by the experiment published by Mr. W. Mathews in the Number of 'Nature' for the 24th of March last. In ascertaining that a thick plank of ice supported at both ends subsided about $1\frac{1}{2}$ inch in twenty-four hours, during a sharp frost, under the pressure of its own weight alone, Mr. Mathews did not disclose a fact which ought to excite surprise. So far from being anomalous, this property of ice is in full accordance with what we know of the behaviour of other similar bodies. I am, however, at a loss to understand whether Mr. Croll maintains that the fact can be accounted for only by assuming that each molecule of the ice momentarily became fluid as it shifted its relative position so as to allow of the bending of the ice. If this occurs when the temperature is considerably below the freezing-point, the so-called molecular hypothesis cannot stop here. It must be extended to all bodies which are capable of changing their form under pressure without fracture at temperatures below that of liquefaction. If, on the other hand, Mr. Croll does not consider his hypothesis to be applicable to the case in question, I must ask why he holds it to be requisite in the case of a glacier, where the amount of relative disturbance of the particles is very much less than in the experiment of Mr. Mathews.

I would especially beg Mr. Croll to consider the many grounds, both theoretical and experimental, on which we are led to hold that the temperature of the interior of great glaciers is very nearly, if not absolutely, constant, and how impossible it is to hold that their advance can be dependent on the passage through their mass of any sensible quantity of heat such as could effect the molecular changes which he contemplates.

If the molecular theory of Mr. Croll is no longer to be applied specially to the conduct of ice under pressure, but is extended generally to all, or most, plastic bodies that yield slowly to pressure, I shall merely say that it appears to me to lack antecedent probability, and to be at least premature. In spite of recent progress, our knowledge of the molecular constitution of matter is yet far too imperfect to enable us to reason with any confidence on the general question raised by Mr. Croll. Should he be induced to follow up the inquiry, it may in the hands of so acute a thinker lead to important results; but in the mean

time, so long as the phenomena exhibited by ice are strictly analogous to those offered by other substances of the same class, I shall not think that any special hypothesis is required for their explanation.

In conclusion I venture to state the explanation of the cause of glacier-motion which appears to me to be best consistent with the facts, and which I am content to submit to those who are familiar with the phenomena, without entering into detailed arguments, already well-known to those who have attended to the subject, and which could not be fully set forth without infringing unduly on the space accorded to me.

In the first place, I must dwell for a moment on the fact, familiar yet constantly forgotten, that glacier-ice is a very different substance from the ice that is formed in winter on the surface of lakes and rivers. However slight the external indications of the fact may be, it is a congeries of separate fragments more or less perfectly welded together, but not possessing a uniform or common crystalline structure, and showing by the frequent presence of air-bubbles, and by its behaviour when exposed to radiant heat, an inferior degree of solidity. On this account I objected to Mr. Moscley's experiments on shearing-force, because I held them to be inapplicable to glacier-ice; and I further hold that, if the most careful experimenter were to determine the shearing-force of a given square inch of glacier-ice, it would be unsafe to conclude that the resistance offered by an adjoining square inch, or a number of other equal sections of the glacier, would be found nearly equal.

I further note, as an essential characteristic of the great glaciers whose onward motion is rapid, that they are traversed by fissures often of great width, and of depth great even relatively to the dimensions of the glacier.

Finally, I consider it to be an established fact that all considerable glaciers, even those of the second order when they lie below what is called the snow-line, have a fixed internal temperature of 32° Fahr., from which they never vary by more than a minute fraction, and that the influence of the seasons, still more that of day and night, penetrates but a very moderate distance from their exterior surfaces.

Glacier-ice is a substance which at the temperature of freezing is capable of yielding, very slowly, to moderate pressure. A portion of the motion of all glaciers, and the whole, or nearly the whole, of the motion of glaciers of the second order (that is to say, of those which lie on the slopes of mountains, but do not fill a definite trough or valley-channel) is due to this cause, and is effected independently of fracture and regelation (if the former

term is limited to disruption of a sensible or measurable amount*). In the larger glaciers, and especially those which lie in a deep trough and whose bed is very irregular, a new phenomenon comes into play. Glacier-ice, though imperfectly solidified, is yet rigid enough to transmit very considerable pressures; but there is a limit at which pressure upon ice at 32° F. has the effect of reducing it to the liquid state. At any given moment of the progress of a great glacier, especially in summer, certain points are subjected to enormous pressure. A given square inch in the section of the glacier, instead of sustaining, as Mr. Moseley assumes, merely the pressure of a parallelopiped of equal section lying behind it, may at a given moment have to bear the pressure due to a mass a hundred or a thousand times as great in section. The effect may either be that fracture ensues at that point, whereupon the pressure is transmitted to another adjoining point, and so further; or else the pressure liquefies a portion of the ice: the water, even if it cannot escape, occupies less space than it did before; so that the effect of transferring the maximum pressure from one point to another is accomplished. It is this process that I ventured to compare to the progress of a huge snake, whose movements are effected not by simultaneous effort at every point, but by the transmission of muscular energy from one point to another; and many who have happened to traverse a great glacier when its motion was at the fastest, will admit that the words which I added, "straining and groaning audibly"—will allow that these are scarcely figurative terms.

Having failed to explain with sufficient clearness what I believe to be the nature of the process, I am not surprised that Mr. Croll fails to see that it is quite consistent with my views that a glacier should advance more rapidly in a sinuous and irregular channel than in one of uniform width and slope—because in the former case the irregular distribution of pressure more surely determines the yielding of the mass at one point, and thus leads to the transference of the maximum pressure throughout many successive points.

The fact that a rise in external temperature causes a notable increase in the rate of advance of a glacier has been familiar to glacier-theorists since the earliest observations of the late Professor Forbes; but I fail to see that this militates in favour of

* I have admitted that, in my opinion, molecular science is too little advanced to allow us to give a peremptory negative to Mr. Croll's hypothesis as to the passage of heat through ice. If the rein be given to conjecture, I think it more reasonable to suppose that, in all bodies that change their form slowly under pressure, the actual process by which the particles change their relative positions is nearly akin to, if not identical with, that now well known as fracture and regelation.

the views of Mr. Croll, if he admits, as I think all must admit, that the temperature of the interior of the glacier is constant; and especially when it is coupled with the other fact, that a glacier continues to advance in winter, even at times when, owing to the absence of a snow covering, the temperature of the superficial stratum is considerably below the freezing-point.

Admitting that other causes, and especially the infiltration of surface-water, may have a small share in the result, I think the main point to be considered is this: the condition of the glacier is such that, if we consider the superficial stratum to be absolutely neutral—or, in other words, that the forces acting on it are such as to make it move exactly at the same rate as the next underlying portion—a certain not inconsiderable rate of motion is attained. In such a state of things let a change take place in the condition of the superficial stratum by which the forces that resist its onward movement are very much diminished; it is obvious that by its weight and by the cohesion existing between it and the remainder of the mass, this will tend to drag the lower portion forward. As far as we can judge of the nature of plastic bodies, it would appear that the resistance which they offer to change of form diminishes in a ratio far more rapid than the increase of pressure. Their molecular condition is in a state of unstable equilibrium: at a given moment certain pressures are balanced by certain resistances; but a slight increment of pressure may overcome the resistance and cause change in the relative position of the particles. This, as I believe, is the true explanation of the effect of warm weather on advancing glaciers; and the effect is aided by the fact that through open crevasses the heat often penetrates partially into the interior. Conversely, winter cold increases to a slight extent the resistance to the onward motion of a glacier, by making the superficial stratum rather less ready to yield than the interior of the glacier, so that the rate of advance is then reduced somewhat below what may be termed its normal velocity, namely that which it would obtain if the influence of the superficial stratum were absolutely removed.

XIV. *On the Chemical Composition and Microscopic Constitution of certain Cornish Rocks.* By J. ARTHUR PHILLIPS, F.C.S., M. Inst. C.E., &c.*

THE County of Cornwall forms a peninsula principally consisting of "*killas*," a name applied by Cornish miners to all sedimentary cleavable rocks. It is difficult to determine the geological age of these, further than that they belong to the palæozoic era; and it is believed, from the rare and imperfectly

* Communicated by the Author.

preserved fossils which have been discovered, that the majority of them are of Devonian age.

This extensive region of sedimentary rocks encloses five large and several smaller masses of granite, which, without rising very prominently above the common level, protrude as "islands of granite out of a sea of slate" *.

Both the slates and granites of this portion of Great Britain are traversed by numerous dykes of granitic and porphyritic rock called *elvans*, by occasional trap-dykes, by numerous copper and tin lodes, and by various cross-courses and *flucans*, the latter being fissures filled with clay.

This formation is characteristic, not only of the whole of Cornwall, but also of a large portion of Devonshire. Masses of serpentine occur in various localities in this district, particularly in the south-western portion of Cornwall, in the vicinity of the Lizard Point.

Typical killas is a clay-slate, often of a grey, bluish-grey, or greenish-grey colour, and frequently acquires a brownish-yellow or buff tint, from weathering: its colour, however, varies very considerably in different localities, besides which it is not unfrequently stained by dendritic markings of oxide of iron and other minerals. It is usually exceedingly fissile, the planes of fracture being very commonly much contorted, and presenting smooth unctuous surfaces, not unlike those of faults or fissures called *slickensides*, and apparently produced by friction: killas is often more compact near its junction with the granite than elsewhere, and in such localities, without any well-defined line of demarcation, frequently encloses numerous subordinate layers of granular or semi-crystalline rocks.

Professor John Phillips remarks, with regard to these deposits, "The general impression concerning the schistose rocks of Cornwall is that their mineral composition is a mixture of quartz, felspar, and mica, and so probably is that of most clays and shales" †. He further observes, "It is hazardous to reason on phenomena so remarkable as those of Cornwall without reference to other districts. Nothing but prejudice or indolence will permit geologists acquainted with other districts to neglect the singular and curious facts connected with the Devonshire and Cornish chain. We may freely admit that they in some cases point to agencies not yet familiar to our philosophy; that a full examination of the whole series of granites, porphyries,

* A "Treatise on Ore Deposits," by Bernhard Von Cotta, Professor of Geology in the Royal School of Mines, Freiberg. Translated from the second edition by Frederick Prime, jun., revised by the author, p. 403.

† Manual of Geology, Practical and Theoretical, by John Phillips, M.A., F.R.S., F.G.S., &c., p. 117.

serpentines and killas, and of the disseminated and venigenous minerals in them, will kindle a brilliant light in the most secret laboratory of nature; but one thing is wanting, an exact description of *all the characteristic facts observable in each particular case*, without the adornments of theory or the disarray of new nomenclature”*. Since the foregoing was written (now fifteen years ago) but little has been done with the view of elucidating the composition and constitution of Cornish rocks†. It is therefore hoped that the following results of a series of careful analyses, and of numerous microscopical examinations, may be of some value as a small instalment of that mass of exact information so necessary to a more accurate knowledge of the petrology of one of our most important mining districts.

The methods which have been employed in conducting the various analyses will be found fully described at the end of this paper; and with regard to the sections for microscopical examination, it may be observed that they have been prepared in the usual way, but that the softer varieties of killas require exceedingly careful manipulation. In the case of the more fissile specimens, it was often found very difficult to obtain satisfactory sections at right angles to the cleavage-planes; and even when procured, great care was required in mounting, to prevent their entire disintegration in the balsam employed for that purpose. Each section was examined by the aid of two different arrangements of the instrument. The first, spoken of as “a low power,” magnified 60, and the second, mentioned as “a high power,” about 400 linear. The polariscope could be conveniently used with both. Higher powers were sometimes, but not often, employed.

Killas from Polgooth Mine, adit-level, sp. gr. = 2·60.—This slate is very soft, has a decidedly clayey smell, and adheres slightly when applied to the tongue. Colour generally light grey, in places marked with a darker shade of the same tint, and occasionally slightly tinged with yellow.

This rock was found, on analysis, to have the following composition:—

* *Manual of Geology, ante cit.*, p. 512.

† A valuable paper relating to the constitution of the Cornish granites has been published by the Rev. Samuel Haughton, F.R.S., of Trinity College, Dublin, entitled “Notes of a Comparison of the Granites of Cornwall and Devon with those of Leinster and Mourne,” *Proceedings of the Royal Society*, 1868–69, p. 209. [*Phil. Mag.* April, 1869, p. 306.]

	I.	II.
Water*	4.16	4.00
Silica	60.45	60.39
Titanic acid†21	.21
Alumina	20.67	21.00
Ferric oxide	8.21	8.13
Ferrous oxide	1.91	1.87
Manganoso-manganic oxide43	.39
Lime	1.86	1.56
Magnesia	trace	trace
Potassa74	.80
Soda	1.56	1.54
	100.20	99.89

Thin sections of this slate, when examined by transmitted light under a low power, exhibit no decided evidence of structure, but appear as a milky-white mass in which are disseminated numerous moss-like semi-crystalline markings of a brownish-green colour. They are also observed to be traversed by various fissures, which have become filled by crystalline transparent quartz. When examined by the aid of a high power, this rock is found to be composed of an aggregate of minute granules intimately blended together and without definite outlines. There are also some grains of oxide of iron and other dark markings not sufficiently opaque for this substance; some of these are apparently fragments of hornblende, whilst others are probably patches of a chloritic mineral.

This slate has in fact undergone so great a change, that the individual grains, of which it was originally composed, can only be made out by means of the polariscope. When the prisms are crossed, the mass breaks up into a kind of shadowy mosaic of light and dark or variously coloured patches without definite outlines.

Killas from Polgooth Mine, eighty-fathom level. Sp. gr. = 2.74.
—This specimen is much harder than that from the adit-level, and is of a grey colour, strongly marked with reddish brown. Its analysis gave the following results:—

* Of which 1.00 was lost in the water-bath.

† The amount of titanic acid in these rocks being exceedingly small, it was determined by weighing the total quantity obtained from both analyses. This having been divided by two, accounts for the exact accordance of the figures in the two columns of results.

	I.	II.
Water*	3·66	3·60
Silica	63·00	63·20
Titanic acid	trace	trace
Phosphoric acid	trace	trace
Alumina	20·50	19·80
Ferric oxide	3·56	3·46
Ferrous oxide	3·10	2·83
Oxide of manganese	trace	trace
Lime	1·35	1·20
Magnesia	trace	trace
Potassa	·95	·95
Soda	3·07	3·22
	<hr/> 99·19	<hr/> 98·26

When microscopically examined, this rock does not differ materially from that obtained nearer the surface, excepting that the grains of oxide of iron are larger but less numerous, and that it contains fewer of what appear to be broken crystals of a hornblendic mineral. The granular fragments of which it is composed are also larger and more distinct than in the former case; and on examination of transverse sections it becomes evident that, in the majority of instances, their larger surfaces are nearly parallel with the planes of cleavage of the slate†. The chloritic mineral, mentioned as occurring in killas from the adit-level, is also very abundant; and about the margin of many of the patches of this substance peculiar markings are observed. These, which may perhaps be the result of forces producing cleavage, will be more fully noticed when describing the appearance of sections of clay-slate from Polmear Mine.

Killas from Polgooth Mine, one-hundred-fathom level. Sp. gr. =2·73.—The killas at this depth is of a greenish-grey colour, slightly streaked and spotted with yellow, and is rather harder than that found at shallower levels. Its chemical composition was found to be as follows:—

	I.	II.
Water‡	3·33	3·20
Silica	51·00	50·83
Titanic acid	trace	trace
Alumina	20·67	20·90
Ferric oxide	13·44	13·39
Ferrous oxide	4·70	5·14
Oxide of manganese	trace	trace
Lime	1·68	1·56
Potassa	·95	·91
Soda	3·96	4·20
	<hr/> 99·73	<hr/> 100·13

* Of which ·66 was lost in the water-bath.

† This was observed to be the case in all the specimens of clay-slate which were examined.

‡ Of which 2·00 was lost in the water-bath.

Sections prepared from specimens of rock taken from this depth very closely resemble those from the eighty-fathom level; no fragments of hornblendic crystals, however, were observed, and chloritic patches were less abundant.

Killas from Polmear Mine, forty fathoms below surface. Sp. gr. = 2·68.—This specimen of clay-slate is much contorted, and readily divides into curved laminae with glossy surfaces, on which were numerous wavy lines resembling minute ripple-marks. Its colour is a medium shade of grey, in places slightly marked with yellow. Its composition, as found by analysis, is as follows:—

	I.	II.
Water*	6·66	6·50
Silica	49·33	49·20
Titanic acid	trace	trace
Alumina	18·00	18·00
Ferric oxide	12·63	12·73
Ferrous oxide	8·56	8·54
Ferric persulphide	·80	·82
Lime	2·14	2·12
Magnesia	trace	trace
Potassa	·57	·56
Soda	·75	·74
	99·44	99·21

Sections prepared from this rock do not differ materially from those obtained from the Polgooth specimens. The ultimate fragments of which it is composed are exceedingly minute; but granules of quartz of an appreciable size, and affording colours with polarized light, are disseminated throughout the fine-grained matrix. A greenish tint is also imparted to it by chlorite; and it is minutely divided by a system of markings made up of pairs of nearly parallel lines, each about $\frac{1}{1000}$ of an inch in length and $\frac{1}{8000}$ of an inch apart. These cross each other so thickly as to form a kind of close network, and give no colours by polarized light: they were at first taken for amorphozoa; but my friend Mr. R. Etheridge, who has kindly examined them for me, is of opinion that they are “certainly not organic.”

Slate from “Sanctuaries” near St. Austell. Sp. gr. = 2·52.—This, in common with many other Cornish slates lying above the natural drainage-level of the country, is very light in colour, the prevailing tints being light grey and buff, and bears the appearance of weathering. It adheres strongly to the tongue, has a decidedly clayey smell when freshly broken, and divides with difficulty into laminae, of which the surfaces are entirely without polish. Its specific gravity is low; and it is divided by headings variously coloured, apparently by infiltration of water containing oxide of iron.

* Of which ·93 was lost in the water-bath.

For the purpose of analysis, fragments of a pure grey colour, as far as possible free from stains of iron, were chosen. The following results were obtained:—

	I.	II.
Water*	6·16	6·16
Silica	53·40	53·20
Titanic acid	trace	trace
Phosphoric acid . .	trace	trace
Alumina	21·39	22·06
Ferric oxide	6·25	5·78
Ferrous oxide	4·28	4·28
Lime	trace	trace
Magnesia	·75	·75
Potassa	2·90	2·94
Soda	4·34	4·05
	<hr/> 99·47	<hr/> 99·22

No sections of this rock were made.

Killas from Dolcoath Mine†, two-hundred-and-fifteen-fathom level. Sp. gr. = 2·71.—This celebrated mine, which is situated near the town of Camborne, is worked in killas at a short distance from its junction with the granite. It yields ores of both tin and copper.

The specimen of rock, from which a portion was broken for analysis, is exceedingly hard, and the cleavage imperfect, the prevailing colour being dark grey. When freshly broken the surfaces of fracture exhibit numerous minute, but brilliant, indistinct crystals, probably of some hornblendic mineral. Its composition was found to be as follows:—

	I.	II.
Water‡	1·14	1·16
Silica	67·34	67·29
Titanic acid	·13	·13
Alumina	20·94	20·75
Ferric oxide	2·68	2·99
Ferrous oxide	1·66	1·66
Lime	2·10	1·95
Magnesia	trace	trace
Potassa	·58	·61
Soda	3·34	3·40
	<hr/> 99·91	<hr/> 99·94

When examined by the aid of a low power, sections of this

* Of which ·35 was lost in the water-bath.

† I am indebted to the kindness of Mr. W. J. Henwood, F.R.S., for procuring various specimens of rock from Dolcoath, Botallack, and Delabole. He obtained the first through Mr. Josiah Thomas, Manager of the mine; the second were supplied by Mr. Stephen Harvey James, and those from Delabole by Mr. J. Hockaday, Director of the works.

‡ Of which ·48 was lost in the water-bath.

rock are seen to be chiefly made up of an aggregation of transparent angular particles, giving colours with polarized light; disseminated throughout the mass are numerous shades and patches of a greenish colour, which are probably due to chlorite. Under a high power they are found to contain many black grains of magnetite and titaniferous iron, together with fan-like aggregations of transparent acicular crystals, which often diverge from a particle of oxide of iron as a centre.

Besides these radiated combinations, there are other apparently similar crystals, which cross each other in all directions; these, as well as the former, are probably either hornblende or schorl. There are also some broken and rounded fragments of mica and of a semi-translucent mineral, which is not sufficiently opaque for oxide of iron; these last are mechanically embedded in the slate.

Killas from Botallack, Penzance, surface near lode. Sp.gr. = 2.95. —This rock, which is of a dark greenish-grey colour, sometimes contains minute crystals of iron pyrites, and is very hard; its cleavage has been to a great extent obliterated by metamorphism; but it usually divides, when broken, into roughly tabular masses, of which the planes approximate to those of original cleavage. For analysis, a specimen was chosen free from crystals of pyrites and from ferruginous stains. The following were the results obtained:—

	I.	II.
Water*	3.12	3.13
Silica	40.27	40.16
Titanic acid15	.15
Phosphoric acid†66	.66
Alumina	24.03	23.99
Ferric oxide	4.26	4.16
Ferrous oxide	11.34	11.20
Sulphur	trace	trace
Lime	4.16	4.05
Magnesia	6.46	6.58
Potassa	1.66	1.68
Soda	3.54	3.60
	<hr/> 99.65	<hr/> 99.36

The low power ordinarily employed shows this rock to be composed of an apparently amorphous base, often of a green colour, probably due to chlorite, in which no distinct grains are distinguishable; in this are porphyritically enclosed numerous elongated transparent crystals and crystalline patches, the latter being frequently fringed at the edges by a growth of acicular

* Of which .39 was lost in the water-bath.

† Only one estimation of phosphoric acid was made.

crystals, which give brilliant colours with polarized light. Under a high power the ultimate fragments, of which the slate is composed, are still very indistinct, but some long slender crystals, probably apatite, are distinctly seen; there are also some minute crystals of an apparently triclinic mineral, which may be axinite.

Rock from Botallack, one hundred and thirty fathoms deep. Sp. gr. = 2·82.—This specimen, which was sent under the name of "killas," has lost all traces of cleavage, and has a cross fracture with polished surfaces resembling those so common in many varieties of serpentinous rock*. It has a very dark green colour, with slight traces of a lighter shade of the same tint; and in places minute crystals of pyrites are seen by the aid of a lens. The fragments chosen for analysis were apparently free from these crystals, and afforded the following results:—

	I.	II.
Water†	11·06	11·12
Silica	32·93	33·03
Titanic acid	trace	trace
Phosphoric acid	trace	trace
Alumina	16·69	16·77
Ferric oxide	7·17	6·88
Ferrous oxide	13·67	13·75
Sulphur	trace	trace
Lime	5·02	4·78
Magnesia	11·43	11·61
Potassa	·78	·68
Soda	·64	·61
	99·39	99·23

In this rock, which slightly attracts the magnetic needle, grains of oxide of iron are more numerous than in specimens obtained nearer the surface; the fragments of which it is composed are also larger and more clearly defined. It contains but few long crystals of the kind found in sections procured from the surface rock, but is traversed by minute veins of quartz in which transparent acicular crystals, probably of some hornblendic mineral, are observed. It will be remarked that both the specimens of so-called killas from this mine contain a large amount of magnesia, which is all but entirely wanting in the clay-slates from the neighbourhood of St. Austell. In fact, from its physical constitution, as well as from its chemical composition, the rock from the deeper level may be regarded as an impure serpentine.

Roofing-slate, Delabole. Sp. gr. = 2·81.—The quarries of Delabole are situated near the town of Camelford, and yield large

* From some of these polished surfaces thin plates of calcite were detached.

† Of which 4·12 was lost in the water-bath.

quantities of slates and slabs of good quality, which are extensively used by builders in the west of England.

The roofing-slates of best quality cleave very readily into thin laminae, and are of a grey colour, thickly speckled with a lighter shade of the same. The second-quality slates have a greenish tint, and are spotted with a lighter shade of the same colour; some of the seams are uncleavable, and rendered utterly worthless for roofing-purposes by being much twisted and interfoliated with small lenticular deposits of quartz. Some of the surfaces, and particularly those indicating the planes of bedding, are covered by a thin crystalline film of calcite.

A specimen of "best roofing-slate" afforded by analysis the following percentage results:—

	I.	II.
Water*	4·62	4·60
Silica	58·25	58·35
Titanic acid	·23	·23
Phosphoric acid	trace	trace
Alumina	21·74	22·04
Ferric oxide	7·15	6·96
Ferrous oxide	2·57	2·57
Lime	·40	·39
Magnesia	1·09	1·10
Potassa	2·44	2·45
Soda	1·04	1·23
	99·53	99·92

This slate, under a low power, does not show any distinct structure, but is seen to be thickly dotted with dark spots. When examined by the help of a high power its structure still remains indistinct, but numerous clusters, about $\frac{3}{1000}$ of an inch in diameter, of reddish-brown crystals become distinctly apparent. They are generally opaque; but some of them, which have been ground very thin during the preparation of the sections, appear to result from the aggregation of several hexagonal plates: these crystals are probably micaceous iron†.

In addition to the crystalline nodules above referred to, the matrix of this slate is traversed in all directions, but chiefly lying in planes parallel with the cleavage, by long transparent crystals

* Of which ·35 was lost in the water-bath.

† Having been unable to arrive at a satisfactory conclusion with regard to the exact nature of the dark crystalline bodies occurring in this rock, I forwarded a section to Mr. H. C. Sorby, F.R.S., with a request that he would be kind enough to examine it. After having done so, he writes as follows:—"The dark crystals in the slate from Delabole are similar to what I have seen in many others. As far as I can make out, they are imperfect hexagonal plates; and since the general character clearly shows that they must at all events contain much iron, it seems very probable that they may be more or less altered specular iron. This, of course, would well agree with the fact of its containing titanitic acid."

which are brilliantly coloured by polarized light. Like the brown crystals, occurring in groups, these are not mechanically embedded, but have been formed *in situ* since the deposition of the clayey matter of which the rock is mainly composed.

Diorite, "Sanctuaries," St. Mewan. Sp. gr. = 2·97.—On the map of Cornwall, prepared by the Officers of the Government Geological Survey, a dyke of "greenstone" is laid down as coursing in a north-easterly direction from the farm of Quoit to a point about a quarter of a mile north of the turnpike-road from Truro to St. Austell, where it expands into a large mass at the summit of the hill; thence it extends, in diminished proportions, in a south-easterly direction to the sea at Duporth. This rock, which is of a dark green colour verging on black, is exceedingly hard and tough, and is extensively worked, at the point of its greatest expansion, as a material for making roads. The quarry from which this stone is obtained adjoins the "sanctuary fields" belonging to the glebe of the parish of St. Mewan, and has long afforded a road-material of such remarkable excellence that it is carted to considerable distances to be employed for that purpose.

In the quarry above referred to, this rock is distinctly crystalline, frequently encloses patches of iron pyrites, and attracts the magnetic needle. In some smaller openings which have been made upon it on its south-eastern extension, its crystalline character has to a considerable extent disappeared, and pyrites has been replaced by oxide of iron. Fragments of rock free from pyrites were chosen for analysis; and three separate analyses were made, with the following results* :—

	I.	II.	III.
Water†	·83	·81	·76
Silica	47·66	47·33	47·70
Titanic acid	trace	trace	trace
Phosphoric acid	·16	·18	trace
Alumina	17·50	17·15	16·83
Ferric oxide	12·52	13·18	13·42
Ferrous oxide	9·42	9·42	9·07
Oxide of manganese	trace
Sulphur	trace	trace	trace
Lime	4·20	4·03	4·10
Magnesia	trace	trace	trace
Potassa	2·43	2·33	2·15
Soda	5·19	5·27	5·88
	99·91	99·70	99·91

* Analyses I. and II. were made on the same fragment of rock; No. III. on a different one.

† Of which ·33 was lost in the water-bath.

Thin sections, when examined under the microscope, exhibit unmistakable evidence of extensive alteration. The felspar does not generally present any distinctive characteristics, although in a few instances the parallel striping peculiar to triclinic varieties was observed. In addition to felspar this rock contains numerous semi-transparent yellowish-brown crystals, probably hornblende; there is also a green mineral which has a fibrous structure, showing colours with polarized light; this is believed to be a variety of hornblende.

Many black grains of oxide of iron, and some well-defined hexagonal crystals (which, as phosphoric acid has been shown to be present, are in all probability apatite), are disseminated throughout the mass; there is likewise an abundance of a greenish chloritic mineral, which is doubtless a secondary product. Mr. Samuel Allport, F.G.S., an acknowledged authority on the microscopic structure of rocks, to whom I submitted a section, agrees in the opinion that this is probably a diorite that has undergone much alteration.

Those portions of the eruptive dyke which are in close proximity to, or come into direct contact with, the enclosing slaty rock not unfrequently exhibit a quasi-schistose structure. Sections made from specimens of this description were found to have lost all traces of crystalline arrangement, and to consist of an amorphous felspathic base, in which a greenish chloritic mineral is disposed in parallel bands. Some of the sections which have been prepared from this variety of the rock are traversed by minute veins of crystalline quartz enclosing numerous well-defined crystals of hornblende; and it was observed that the chloritic bands were frequently bent at their point of intersection with the veins of quartz.

"Greenstone," Blowing-House Hill, St. Austell. Sp. gr. = 2.89.

—Some twenty years since, a quarry was worked for road-material immediately below the old schoolroom on Blowing-House Hill, St. Austell. The general appearance of the stone obtained from this locality is very similar to that of the rock from the "Sanctuaries," excepting that it has a somewhat duller aspect, and is divided by a series of oblique cross headings which impart to it a tendency to divide into rhomboidal masses. It is also less decidedly crystalline than the rock previously described; and although in no degree fissile, it nevertheless exhibits a grain resembling that of some varieties of metamorphosed schist. The following results were obtained by analysis:—

	I.	II.
Water*	1.00	1.00
Silica	47.68	47.33
Titanic acid	trace	trace
Phosphoric acid	trace	trace
Alumina	17.13	16.86
Ferric oxide	11.73	11.77
Ferrous oxide	10.71	10.71
Manganoso-manganic oxide.	.42	.40
Lime	6.28	6.29
Magnesia	trace	trace
Potassa	2.94	2.84
Soda	2.53	2.56
	<hr/> 100.42	<hr/> 99.76

It will be observed that the chemical composition of this rock is almost identical with that of the diorite extending from Quoit farm to Duporth, with the exception of its containing an appreciable amount of oxide of manganese, and that the percentage of soda is only about one half of that found in the former case.

Under the microscope it is seen to consist of a compact felspathic matrix, enclosing a few indistinct crystals of felspar, together with large quantities of the greenish chloritic mineral referred to in the description of a diorite from St. Mewan. This last is arranged in approximately parallel bands and patches, many of the latter having the appearance of partially decomposed crystals of hornblende; the central portion of these retain to a certain extent their original structure and colour, whilst the edges have become transformed into chlorite, or some nearly allied mineral.

This rock, which also contains many black grains of oxide of iron, and a few hexagonal crystals, which are perhaps apatite, may be either a metamorphosed slate, or a diorite which has become much changed by the effects of a gradual re-arrangement of its constituents. I however incline to the opinion that it is an altered clay-slate.

Serpentinous rock, Menheniot. Sp. gr. = 2.77.—A cutting of the Cornwall railway intersects, at the Menheniot station, a wide band of a dark green rock which is there extensively quarried for road-making. Its total width may be taken at about fifty yards; but it is more or less intermixed with the enclosing clay-slate, from which it is separated by no well-defined walls or lines of demarcation, since the two gradually merge into each other; and beds of unaltered slate are found interstratified with the serpentinous greenstone. Its general structure is decidedly

* Of which .33 was lost in the water-bath.

schistose ; and it exhibits numerous resinous headings, of the kind which is so characteristic of rocks of this class. Many of these are coated by asbestos, whilst in some of the fissures, and particularly in the cross headings, crystalline calcite is of frequent occurrence. Saponite is also present in the form of reniform concretions, which are most frequently found in a clay, apparently resulting from the decomposition of the principal rock. This stone slightly affects the magnet, is only moderately hard, and when recently broken is of a dark green colour with a somewhat crystalline fracture. Its chemical composition was found to be as follows :—

	I.	II.
Water*	10·66	10·46
Silica	38·60	38·80
Titanic acid	trace	trace
Phosphoric acid	trace	trace
Alumina	17·58	17·60
Ferric oxide	14·98	15·10
Ferrous oxide	4·62	4·50
Chromic oxide (Cr^2O^3)†	·14	·14
Oxide of manganese	trace	trace
Lime	5·04	4·92
Magnesia	5·97	6·04
Potassa	trace	trace
Soda	·84	·85
	<hr/> 98·43	<hr/> 98·41

The microscope shows this to be a highly metamorphosed rock, consisting of an amorphous matrix porphyritically enclosing yellowish-brown or green patches with indistinct crystalline forms, which are evidently pseudomorphs. There are also many black grains of magnetite, and crystals of some pyroxenic mineral—probably schiller-spar or diallage.

This appears to afford an illustration of the gradual production of serpentine, by the absorption of magnesia &c., by stratified and other rocks ; in the same way the analyses of the two specimens of killas from Botallack would seem to indicate that some of the clay-slates in that district may have become to a certain extent affected by a serpentinous metamorphism ‡.

* Of which 2·17 was lost in the water-bath.

† Only one estimation made.

‡ As an instance of the comparatively recent production of a magnesian mineral by the agency of water, it may be mentioned that at the mining town of Washington, California, some of the pebbles of an ancient river-bed of post-tertiary age have become embedded in asbestos, in the form of long, easily separable crystals. This is probably the result of metamorphic action on a plastic clay in which the boulders were originally enclosed, and

Serpentine, Lizard. Sp. gr. = 2·59.—The specimen selected for analysis was broken from a slab prepared for decorative purposes, and is of a very dark green colour, in places verging on black; it is thickly spotted with red, and has a coarsely granular structure. This serpentine is massive, without any indication of foliation, and encloses imperfectly defined crystals presenting brilliant surfaces when broken through.

Its chemical composition was found to be as follows:—

	I.	II.
Water*	15·52	15·52
Silica	38·86	38·58
Alumina	2·95	3·06
Ferric oxide	1·86	1·95
Ferrous oxide	5·04	5·10
Oxide of manganese	trace	trace
Oxide of nickel (NiO)	·28	·30
Chromic oxide (Cr ² O ³)	·08	·08
Lime	trace	trace
Magnesia	34·61	34·32
Potassa	·33	·30
Soda	·77	·76
	100·30	99·97

The microscope shows this rock to consist of a cryptocrystalline base, spotted by oxide of iron &c., and enclosing indistinct green or yellowish-brown crystalline forms—pseudomorphs after pyroxene? These crystals give irregular colours with polarized light.

Orthoclase, Roche. Sp. gr. = 2·55.—On the right-hand side of the road leading from St. Austell to the village of Roche, and at a distance of a mile from the latter place, a deposit of yellowish-white crystalline felspar is quarried in the bottom of a shallow ravine.

The area over which this mineral extends has not been accurately determined; but it has been worked, on the course of the hollow, by means of shallow pits, for a length of about one hundred yards, and to a width of some sixty feet. It is enclosed in granite containing considerable quantities of schorl, and is tra-

which in other portions of the same deposit remains unchanged, with the exception of having become hardened into a kind of stone.

This asbestos, which is found for a distance of several yards on either side of a band of schistose serpentine crossing the valley nearly at right angles, has evidently been produced *in situ*, and encloses pepitas of water-worn gold, together with fragments of wood, which are still readily cut with a knife. I visited this locality, and carefully examined the conglomerate in question, during the fall of the year 1866.

* Of which 2·06 was lost in the water-bath.

versed in all directions by veins of milky quartz, some of which afford good specimens of tourmaline and of well-crystallized cassiterite, which is usually associated with the former; crystals of tourmaline are found in the felspar, as well as in the veins of quartz by which it is intersected.

These workings are locally known as the "Glass Mine;" and the felspar, after being separated from quartz and fragments stained by oxide of iron, by careful hand-picking, is shipped for use in the manufacture of certain descriptions of pottery.

Professor W. H. Miller, F.R.S., of Cambridge, who kindly made for me a crystallographic examination of this mineral, found it difficult to obtain very accurate measurements, on account of the imperfection of the faces, but pronounced it to be a monoclinic felspar.

Its chemical composition was found to be as follows:—

	I.	II.	Oxygen ratio of mean.
Water*	·83	·50	
Silica	65·00	65·33	34·84
Alumina	19·00	19·16	9·10
Ferric oxide	·50	·50	·15
Lime	1·57	1·68	·48
Magnesia	trace	trace	
Potassa	10·37	10·37	1·76
Soda	2·40	2·40	·62
	<u>99·67</u>	<u>99·94</u>	

From the above ratio we obtain the following:—

Silica	12·18
Sesquioxides	3·19
Protoxides	1·00

This is manifestly in the proportion of 1 : 3 : 12; and the felspar is consequently orthoclase.

In making the foregoing analyses the following general routine was adopted.

The amount of moisture present was ascertained by heating a weighed quantity of the finely pulverized rock in a water-bath until it ceased to lose weight. Another weighed portion was moderately ignited, in a platinum crucible, in one of Griffin's gas-furnaces, and the loss of weight noted.

About thirty grains, more or less, of the finely powdered rock was intimately mixed, in a platinum crucible, with from five to six times its weight of pure carbonate of sodium, and fused at a high temperature in a gas-furnace.

* Of which ·33 was lost in the water-bath.

The fused mixture was treated with dilute hydrochloric acid and subsequently evaporated to dryness. The dried residuum was then moistened with hydrochloric acid, water added, and solution of the chlorides assisted by the application of heat. *Silica (a)*, now separated by filtration, was washed, dried, and ignited.

After adding a few drops of nitric acid to the filtrate from silica, for the purpose of ensuring the peroxidation of all the iron, it was evaporated to dryness, treated with dilute hydrochloric acid, and filtered, thus separating a small additional quantity of *silica (b)*.

The filtrate from the second evaporation, after separation of silica *b*, was rendered nearly neutral by ammonia, and acetate of sodium added. The solution was then briskly boiled, and the precipitated basic acetates of iron and aluminium were collected and washed. This precipitate was dissolved in hydrochloric acid, a few drops of nitric acid added, and, after cooling, re-precipitated by ammonia, washed, dried, ignited, and weighed. After weighing, this was dissolved in hydrochloric acid, and a third small portion of *silica (c)* obtained by filtration.

The iron in the filtrate from silica *c* was reduced to the state of ferrous oxide by the introduction of weighed pieces of metallic zinc; and after making due allowance for the amount of that metal contained in the zinc added, the quantity of iron present was ascertained by Penny's process. This, after being transformed by calculation into ferric oxide, was subtracted from the united weight of ferric oxide and alumina before found (less silica *c*), the difference in weight being regarded as *alumina**.

* When phosphoric acid was found in a rock, the amount of that substance as well as that of ferric oxide, indicated by volumetric estimation, was deducted from the united weights of ferric oxide and alumina, as determined by the balance, and the difference was regarded as alumina.

For the sake of checking the accuracy of the results obtained in estimating alumina by difference, the ferric oxide and alumina were in several instances redissolved in hydrochloric acid, and the solution treated with an excess of pure hydrate of sodium in a platinum dish, and, after boiling, the ferric oxide was separated by filtration. The ferric oxide was then redissolved, precipitated by ammonia, washed, dried, and weighed.

The filtrate containing alumina, in solution in caustic soda, was afterwards acidified with hydrochloric acid, chloride of potassium was added to destroy organic matter, the solution boiled, and the alumina precipitated by ammonia and carbonate of ammonium, washed, dried, and weighed.

In all cases, however, it was found necessary to re-dissolve the oxide of iron twice, and to treat the solution each time with hydrate of sodium in order to obtain satisfactory results. Even when this was done, the quantity of ferric oxide found was invariably slightly in excess of that indicated by volumetric analysis; and although this difference seldom exceeded two-tenths of one per cent., the volumetric process was finally adopted, as being both more accurate and more expeditious.

In order to determine the respective amounts of *ferrous* and *ferric oxides* present, separate fusions were made, in a covered platinum crucible, of weighed quantities of pulverized rock mixed with six times their weight of carbonate of sodium, the surface of the mixture being covered by a thick layer of pure carbonate of sodium.

The contents of the crucible were thus both fused and allowed to cool in an atmosphere of carbonic acid, produced by the decomposition of the alkaline carbonate by silica and silicates, besides being preserved from oxidation by a fused covering of carbonate of sodium.

The crucible and its contents were now introduced into a beaker containing hot dilute hydrochloric acid which had been previously boiled, and the whole covered by a glass plate. As soon as bubbles of carbonic acid had ceased to be evolved the solution was allowed to cool, and the amount of ferrous oxide estimated by a standardized solution of bichromate of potassium.

The ferrous oxide thus found was subsequently converted by calculation into $\text{Fe}^2 \text{O}^3$, and deducted from the total amount of iron estimated as ferric oxide.

The *lime* was obtained from the filtrate from the precipitate by acetate of sodium, by the addition of ammonia and oxalate of ammonium, and was weighed as carbonate of calcium. To the filtrate from the calcium salt, phosphate of sodium was added for the precipitation of *magnesia* as pyrophosphate of magnesium. *Titanic acid* was estimated by the process recommended by Riley*, which was conducted as follows.

The silica (*a*, *b*, and *c*) from both analyses was attacked in a platinum dish by hydrofluoric acid, and sulphuric acid added, evaporated to dryness, and ignited. In this way a residue was obtained, which was fused with bisulphate of potassium and dissolved in cold water; the liquid after complete solution was boiled; on boiling briskly for some time a precipitate was formed, which was separated by filtration, washed, dried, and ignited. This precipitate gave the characteristic reactions of titanic acid, and was deducted from the total weight of silica previously determined. The quantity of titanic acid in these rocks is so exceedingly small, that it was found no appreciable quantity could be obtained by re-solution of the iron, separated from alumina by caustic potassa, boiling with sulphite of sodium, &c., as recommended by Riley. Care was therefore taken to evaporate twice to dryness from silica; and any traces of titanic acid that might be retained by the iron (after separation of silica *c*) were, excepting in the analyses of slate from Delabole, neglected.

It having been found that the whole of the phosphoric acid

* Quarterly Journal of the Chemical Society, vol. xii. p. 13.

present in these rocks was soluble when the finely divided substance was digested with nitric acid, the following process was adopted for its estimation.

About 100 grains of the finely pulverized rock was attacked by nitric acid in a platinum dish, and evaporated nearly to dryness to remove the excess of acid. The insoluble matters were removed by filtration, and the filtrate reduced to a very small bulk by evaporation; molybdate of ammonium was then added and the beaker, carefully covered, set aside in a warm place for twenty-four hours to allow of the formation of a precipitate.

The precipitate (when one was obtained) was thrown on a filter and washed with cold water, to which had been added molybdate of ammonium and a little nitric acid.

Strong ammonia was now poured on the filter, and the filtrate evaporated to dryness in order to render insoluble any silica which, at the same time as the yellow precipitate, might have been dissolved by ammonia. The residue was then taken up by hydrochloric acid, insoluble matter separated by filtration, and ammonia and magnesium mixture added. After being set aside for twelve hours to allow of the formation of the magnesium-salt, the clear liquor was drawn off by a pipette, the precipitate re-dissolved by a few drops of hydrochloric acid, and re-precipitated by the addition of ammonia. This precipitate was washed, dried, ignited, and weighed as pyrophosphate of magnesium, from the weight of which the amount of *phosphoric acid* was calculated.

When the rock under examination was found to contain an appreciable amount of *manganese* or (as in the case of the serpentine) *oxide of nickel*, the general routine of analysis required the following modification.

The filtrate from silica was neutralized by ammonia, chloride of ammonium added, and finally sulphide of ammonium. The precipitate thus produced was dissolved in hydrochloric acid to which a few drops of nitric acid had been added, evaporated to dryness for the purpose of separating traces of silica, and re-solution effected in hydrochloric acid. This, after filtration, was treated by carbonate of barium, by which oxide of iron and alumina were precipitated*; these were subsequently estimated in the usual way.

* In order to remove any sulphuric acid resulting from the action of nitrohydrochloric acid on the sulphides, a few drops of chloride of barium were added to the filtrate before the addition of carbonate of barium. In the event of a trace of alumina having escaped precipitation by carbonate of barium, it is readily separated from oxide of manganese or oxide of nickel by digestion in an excess of hydrate of potassium or hydrate of sodium.

The oxide of manganese, or oxide of nickel, was obtained from the filtrate from oxide of iron and alumina and excess of carbonate of barium, after the removal of baryta in the form of sulphate of barium.

Oxide of chromium was estimated by fusing in a platinum crucible about 100 grains of pulverized rock with four times its weight of carbonate of sodium, and half that quantity of nitrate of potassium.

The fused mass was then treated by boiling water, a certain amount of alcohol added, and the whole heated gently for several hours. The insoluble matter was subsequently separated by filtration, acetic acid in excess added to the filtrate, and the chromium precipitated as chromate of lead by the addition of a solution of the acetate of that metal.

The estimation of *sulphur* was effected by attacking a weighed portion of the pulverized rock with nitrohydrochloric acid and, after removing the insoluble matter by filtration, precipitating the sulphuric acid as sulphate of barium; from the weight of this the amount of sulphur originally present was calculated.

In the earlier analyses the *alkalies* were estimated by fusion with baryta in the usual way; but after having made several estimations of potassa and soda in the same rock by the two processes, the following was ultimately adopted as being more expeditious and at the same time equally accurate.

A convenient weight (from 30 to 35 grains) of the finely powdered rock was thoroughly attacked in a platinum dish by strong hydrofluoric acid, and, after the addition of sulphuric acid, evaporated to dryness and ignited over a gas-flame; the residue was now treated with distilled water which was well boiled, taking care to break down all lumps by means of a glass rod.

The whole was transferred without filtration to a large beaker, and excess of ammonia and carbonate of ammonium added.

After the separation by filtration of insoluble matter, and of the precipitate by ammonia and carbonate of ammonium, the filtrate was evaporated to dryness and the ammoniacal salts expelled by ignition. The residue was treated with sulphuric acid, and again evaporated to dryness and ignited to drive off excess of sulphuric acid, dissolved in water, baryta-water added in excess, and filtered. The filtrate was acidified with acetic acid, evaporated to dryness, ignited, and the carbonate of barium and charcoal separated by filtration; the alkaline carbonates in the filtrate were now decomposed by hydrochloric acid, and the whole evaporated to dryness. A few drops of solution of carbonate of ammonium were then added, and the platinum dish strongly heated to drive off ammoniacal salts. The residue was treated with water, any insoluble matter separated by filtration, and the

filtrate evaporated to dryness, gently ignited, and the mixed chlorides weighed.

The separation of potassa and soda was effected in the usual way by means of chloride of platinum.

XV. *On the Light from the Sky, its Polarization and Colour.*
By the Hon. J. W. STRUTT, Fellow of Trinity College, Cambridge*.

IT is now, I believe, generally admitted that the light which we receive from the clear sky is due in one way or another to small suspended particles which divert the light from its regular course. On this point the experiments of Tyndall with precipitated clouds seem quite decisive. Whenever the particles of the foreign matter are sufficiently fine, the light emitted laterally is blue in colour, and, in a direction perpendicular to that of the incident beam, is *completely polarized*.

About the colour there is no *prima facie* difficulty; for as soon as the question is raised, it is seen that the standard of linear dimension, with reference to which the particles are called small, is the wave-length of light, and that a given set of particles would (on any conceivable view as to their mode of action) produce a continually increasing disturbance as we pass along the spectrum towards the more refrangible end; and there seems no reason why the colour of the compound light thus scattered laterally should not agree with that of the sky.

On the other hand, the direction of polarization (perpendicular to the path of the primary light) seems to have been felt as a difficulty. Tyndall says, "... the polarization of the beam by the incipient cloud has thus far proved itself to be *absolutely independent of the polarizing-angle*. The law of Brewster does not apply to matter in this condition; and it rests with the undulatory theory to explain why. Whenever the precipitated particles are sufficiently fine, no matter what the substance forming the particles may be, the direction of maximum polarization is at right angles to the illuminating beam, the polarizing angle for matter in this condition being invariably 45° . This I consider to be a point of capital importance with reference to the present question"†. As to the importance there will not be two opinions; but I venture to think that the difficulty is entirely imaginary, and is caused mainly by misuse of the word reflection. Of course there is nothing in the etymology of reflection or refraction to forbid their application in this sense; but the words have acquired technical meanings, and become associated with certain well-known laws called after them. Now a moment's consideration of the principles according to which reflection and

* Communicated by the Author.

† Phil. Mag. S. 4. vol. xxxvii. p. 388.

refraction are explained in the wave theory is sufficient to show that they have no application unless the surface of the disturbing body is larger than many square wave-lengths; whereas the particles to which the sky is supposed to owe its illumination must be *smaller* than the wave-length, or else the explanation of the colour breaks down. The idea of polarization by reflection is therefore quite out of place; and that "the law of Brewster does not apply to matter in this condition" (of extreme fineness) is only what might have been inferred from the principles of the wave theory.

Nor is there any difficulty in foreseeing what, according to the wave theory, the direction of polarization ought to be. Conceive a beam of plane-polarized light to move among a number of particles, all small compared with any of the wave-lengths. The foreign matter, if optically denser than air, may be supposed to *load* the æther so as to increase its *inertia* without altering its resistance to distortion, provided that we agree to neglect effects analogous to chromatic dispersion. If the particles were away, the wave would pass on unbroken and no light would be emitted laterally. Even with the particles retarding the motion of the æther, the same will be true if, to counterbalance the increased inertia, suitable forces are caused to act on the æther at all points where the inertia is altered. These forces have the same period and *direction* as the undisturbed luminous vibrations themselves. The light actually emitted laterally is thus the same as would be caused by forces exactly the opposite of these acting on the medium otherwise free from disturbance; and it only remains to see what the effect of such forces would be.

On account of the smallness of the particles, the forces acting throughout the volume of any one are all of the same intensity and direction, and may be considered as a whole. The determination of the motion in the æther, due to the action of a periodic force at a given point, requires, of course, the aid of mathematical analysis; but very simple considerations will lead us to a conclusion on the particular point now under discussion. In the first place there is a complete symmetry round the direction of the force. The disturbance, consisting of transverse vibrations, is propagated outwards in all directions from the centre; and in consequence of the symmetry, the direction of vibration in any ray lies in the plane containing the ray and the axis; that is to say, the direction of vibration in the scattered or diffracted ray makes with the direction of vibration in the incident or primary ray the least possible angle. The symmetry also requires that the intensity of the scattered light should vanish for the ray which would be propagated along the axis; for there is nothing to distinguish one direction trans-

verse to the ray from another. We have now got what we want. Suppose, for distinctness of statement, that the primary ray is vertical, and that the plane of vibration is that of the meridian. The intensity of the light scattered by a small particle is constant, and a maximum for rays which lie in the vertical plane running east and west, *while there is no scattered ray along the north and south line.* If the primary ray is unpolarized, the light scattered north and south is entirely due to that component which vibrates east and west, and is therefore *perfectly polarized*, the direction of its vibration being also east and west. Similarly any other ray scattered horizontally is perfectly polarized, and the vibration is performed in the horizontal plane. In other directions the polarization becomes less and less complete as we approach the vertical, and in the vertical direction itself altogether disappears.

So far, then, as disturbance by very small particles is concerned, theory appears to be in complete accordance with the experiments of Tyndall and others. At the same time, if the above reasoning be valid, the question as to the direction of the vibrations in polarized light is decided in accordance with the view of Fresnel. Indeed the observation on the plane of polarization of the scattered light is virtually only another form of Professor Stokes's original test with the diffraction-grating. In its present shape, however, it is free from certain difficulties both of theory and experiment, which have led different physicists who have used the other method to contradictory conclusions. I confess I cannot see any room for doubt as to the result it leads to*.

The argument used is apparently open to a serious objection, which I ought to notice. It seems to prove too much. For if one disturbing particle is unable to send out a scattered ray in the direction of original vibration, it would appear that no combination of them (such as a small body may be supposed to be) could do so, at least at such a distance that the body subtends only a small solid angle. Now we know that when light vibrating in the plane of incidence falls on a reflecting surface at an angle of 45° , light is sent out according to the law of ordinary

* I only mean that *if* light, as is generally supposed, consists of transversal vibrations similar to those which take place in an elastic solid, the vibration must be normal to the plane of polarization. There is unquestionably a formal analogy between the two sets of phenomena extending over a very wide range; but it is another thing to assert that the vibrations of light are really and truly to-and-fro motions of a medium having mechanical properties (with reference to small vibrations) like those of ordinary solids. The fact that the theory of elastic solids led Green to Fresnel's formulæ for the reflection and refraction of polarized light seems amply sufficient to warrant its employment here, while the question whether the analogy is more than formal is still left open.

reflection, whose direction of vibration is perpendicular to that in the incident ray. And not only is this so in experiment, but it has been proved by Green* to be a consequence of the very same view as to the nature of the difference between media of various refrangibilities as has been adopted in this paper. The apparent contradiction, however, is easily explained. It is true that the disturbance due to a foreign body of any size is the same as would be caused by forces acting through the space it fills in a direction parallel to that in which the primary light vibrates; *but these forces must be supposed to act on the medium as it actually is—that is, with the variable density.* Only on the supposition of complete uniformity would it follow that no ray could be emitted parallel to the line in which the forces act. When, however, the sphere of disturbance is small compared with the wave-length, the want of uniformity is of little account, and cannot alter the law regulating the intensity of the vibration propagated in different directions.

Having disposed of the polarization, let us now consider how the intensity of the scattered light varies from one part of the spectrum to another, still supposing that all the particles are many times smaller than the wave-length even of violet light. The whole question admits of analytical treatment; but before entering upon that, it may be worth while to show how the principal result may be anticipated from a consideration of the *dimensions* of the quantities concerned.

The object is to compare the intensities of the incident and scattered ray; for these will clearly be proportional. The number (i) expressing the ratio of the two amplitudes is a function of the following quantities:— T , the volume of the disturbing particle; r , the distance of the point under consideration from it; λ , the wave-length; b , the velocity of propagation of light; D and D' , the original and altered densities: of which the first three depend only on space, the fourth on space and time, while the fifth and sixth introduce the consideration of mass. Other elements of the problem there are none, except mere numbers and angles, which do not depend on the fundamental measurements of space, time, and mass. Since the ratio i , whose expression we seek, is of no dimensions in mass, it follows at once that D and D' only occur under the form $D:D'$, which is a simple number and may therefore be omitted. It remains to find how i varies with T , r , λ , b .

Now, of these quantities, b is the only one depending on time; and therefore, as i is of no dimensions in time, b cannot occur in its expression. We are left, then, with T , r , and λ ; and from what we know of the dynamics of the question, we

* Camb. Phil. Trans. vol. vii.

may be sure that i varies directly as T and inversely as r , and must therefore be proportional to $T \div \lambda^2 r$, T being of three dimensions in space. In passing from one part of the spectrum to another λ is the only quantity which varies, and we have the important law:—

When light is scattered by particles which are very small compared with any of the wave-lengths, the ratio of the amplitudes of the vibrations of the scattered and incident light varies inversely as the square of the wave-length, and the intensity of the lights themselves as the inverse fourth power.

I will now investigate the mathematical expression for the disturbance propagated in any direction from a small particle which a beam of light strikes.

Let the vibration corresponding to the incident light be expressed by $A \cos \frac{2\pi}{\lambda} bt$. The acceleration is

$$-A \left(\frac{2\pi}{\lambda} b \right)^2 \cos \frac{2\pi}{\lambda} bt;$$

so that the force which would have to be applied to the parts where the density is D' , in order that the wave might pass on undisturbed, is, per unit of volume,

$$-(D' - D)A \left(\frac{2\pi b}{\lambda} \right)^2 \cos \frac{2\pi}{\lambda} bt.;$$

To obtain the total force which must be supposed to act over the space occupied by the particle, the factor T must be introduced. The opposite of this conceived to act at O (the position of the particle) gives the same disturbance in the medium as is actually caused by the presence of the particle. Suppose, now, that the ray is incident along OY , and that the direction of vibration makes an angle α with the axis of x , which is the line of the scattered ray under consideration—a supposition which involves no loss of generality, because of the symmetry which we have shown to exist round the line of action of the force. The question is now entirely reduced to the discovery of the disturbance produced in the æther by a given periodic force acting at a fixed point in it. In his valuable paper “On the Dynamical Theory of Diffraction”*, Professor Stokes has given a complete investigation of this problem; and I might assume the result at once. The method there used is, however, for this particular purpose very indirect, and accordingly I have thought it advisable to give a comparatively short cut to the result, which will be found at the end of the present paper. It is proved that if the total

force acting at O in the manner supposed be $F \cos \frac{2\pi}{\lambda} bt$, the re-

* Camb. Phil. Trans. vol. ix.

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 sulting disturbance in the ray propagated along O X is

$$\zeta = \frac{F \sin \alpha}{4\pi b^2 D r} \cos \frac{2\pi}{\lambda} (bt - r).$$

Substituting for F its value, we have

$$\zeta = A \frac{D' - D}{D} \frac{T \pi'}{r \lambda^2} \sin \alpha \cos \frac{2\pi}{\lambda} (bt - r),$$

an equation which includes all our previous results and more.

One reservation, however, must not be omitted. Since we have supposed the medium uniform throughout, whereas it really has a different density at the place where the force acts, our investigation does not absolutely correspond to the actual circumstances of the case. As before remarked, no error is on that account to be feared in the law determining the intensity of the vibration in different directions; but it is probable that the coefficient, so far as it depends on $D : D'$, may be changed*, and there may be a change in the phase comparable with $\frac{2\pi}{\lambda} \times$ the

linear dimension of the particle, which is of importance when the scattered and primary waves have to be compounded.

So much for a single particle. In actual experiments, as, for instance, with Professor Tyndall's "clouds," we have to deal with an immense number of such particles; and the question now is to deduce what their effect must be from the results already obtained. Were the particles absolutely motionless, the partial waves sent out in any direction from them would have permanent relations as to phase, and the total disturbance would have to be found by compounding the *vibrations* due to all the particles. Such a supposition, however, would be very wide of the mark; for, in consequence of the extreme smallness of λ , the slightest motion of any particle will cause an alteration of phase passing through many periods in a less time than the eye could appreciate. Our particles are, then, to be treated as so many *unconnected* sources of light; and instead of adding the *vibrations*, we must take the *intensities* represented by their squares. Only in one direction is a different treatment necessary, namely along the course of the primary light. I mention this because it would not otherwise appear how the reduction in the intensity of the transmitted light is effected; but we do not require to follow the details of the process, because, when once we know the intensity of the light emitted laterally, the principle of energy will tell us what the primary wave has lost.

* I find that no alteration of any kind is needed.—Jan. 20.

The intensity of the light scattered from a cloud is thus equal to

$$A^2 \frac{(D' - D)^2}{D^2} \sin^2 \alpha \frac{\Sigma T^2}{\lambda^4 r^2}$$

where ΣT^2 is the sum of all the squares of T . If T^2 be understood to denote the mean square of T (*not* the square of the mean value of T), and m be the number of particles,

$$\Sigma T^2 = m \cdot T^2.$$

If the primary light be unpolarized, the intensity in a direction making an angle β with its course becomes

$$A^2 \frac{(D' - D)^2}{D^2} (1 + \cos^2 \beta) \frac{m T^2}{\lambda^4 r^2}$$

Backwards from the cloud the light is thus twice as bright as normally. To the light scattered nearly in the direction of the primary ray our expression does not apply.

Fig. 1.

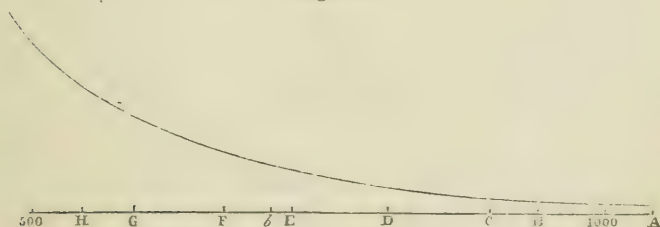


Fig. 1 shows the curve representing the intensity of the scattered light for each part of the spectrum, referred to the intensity in the primary light as a standard. The abscissa being proportional to λ , the base line represents the diffraction-spectrum with the principal fixed lines. Over the brighter portion of the spectrum from B to G the curve differs but little from a straight line, while the small curvature is turned downwards, indicating a deficiency in the green and yellow.

Before making out the theory, I had endeavoured to ascertain by observation the actual prismatic composition of the blue of the sky, and had obtained preliminary results. The experimental method (the description of which I must reserve for another opportunity) was fully adequate to the comparison of two given lights; but the difficulty was to find something to compare the blue light with. In the only complete set of observations that I have hitherto been able to make, the blue of the sky (apparently a very good one) taken from the neighbourhood of the zenith was compared with sunlight diffused through white paper.

About thirty consistent comparisons were made, ranging over the spectrum from C to beyond F, and a curve drawn on the plan of fig. 1. I do not give the complete curve, because I hope before long to complete and confirm the observations; but the following numbers will give an idea of the result:—

C.	D.	b_3 .	F.	
25	40	63	80	from fig. 1.
25	41	71	90	observed.

The upper line gives the theoretical intensities for the fixed lines C, D, b_3 , F, while the lower gives the observed ratios between the lights (sky and diffused sunlight), the two sets of numbers being made to agree at C. Considering the difficulties and uncertainties of the case, the two curves agree very well; and it should be noticed that the sky compared with diffused light was even bluer than theory makes it, on the supposition that the diffused light through the paper may be taken as similar to that whose scattering illuminates the sky. It is possible that the paper was slightly yellow; or the cause may lie in the yellowness of sunlight as it reaches us compared with the colour it possesses in the upper regions of the atmosphere. It would be a mistake to lay any great stress on the observations in their present incomplete form; but at any rate they show that a colour more or less like that of the sky would result from taking the elements of white light in quantities proportional to λ^{-4} . I do not know how it may strike others; but individually I was not prepared for so great a difference as the observations show, the ratio for F being more than three times as great as for C.

There is one point in which our calculations do not exactly meet the case of the sky. In the experiments with precipitated clouds the total quantity of light scattered is quite insignificant compared with the incident beam; but it is by no means so clear that the same is the case with the sky. Each particle is thus struck, not only by the direct light of the sun, but also by that scattered from others. It does not seem that the chromatic effects would be much affected by this consideration; but it is worth notice that the conclusion as to complete polarization perpendicular to the incident ray would have to be modified. To see this, imagine, as before, the light (unpolarized) incident along OY upon a particle O; we have seen that the ray diffracted along OX contains no vibration parallel to OY. By the aid, however, of another particle P in the xy plane such a vibration may be communicated to it; for in the ray diffracted from P to O there is a component vibration in the xy plane perpendicular to PO, which, when again diffracted along OX, will give a component parallel to OY. This is perhaps the explanation

of the incomplete polarization of sky-light at right angles to the solar beams; but it must be remembered that an insufficient fineness in some of the particles of foreign matter would have a like result.

By many physicists, from Newton downwards, the light of the sky has been supposed to be reflected from thin plates, and the colour to be the blue of the first order in Newton's scale. Such a view is fundamentally different from that adopted in this paper, though it might not at first seem so. In support of this assertion, it may be sufficient to notice that the two theories are at variance as to the law connecting the intensity with wave-length. By an argument from dimensions similar to that already used, it is easy to find how the intensity of the light reflected from a thin plate (thin, that is, compared with *any* of the wave-lengths) varies with λ . Instead of our former quantities, T , r , λ , we now have merely λ , and δ the thickness of the plate. Since the reflected vibration necessarily varies as δ , it must also be proportional to λ^{-1} , and so the *intensity* of the reflected light $\propto \lambda^{-2}$ instead of λ^{-4} . The ordinary analytical expression for the reflected light leads readily to the same conclusion (Airy's Tracts, p. 297). There can, I think, be no question that the composition of the light of the sky agrees more nearly with the latter than with the former law.

The principle of energy makes it clear that the light emitted laterally is not a new creation, but only diverted from the main stream. If I represent the intensity of the primary light after traversing a thickness x of the turbid medium, we have

$$dI = -kI\lambda^{-4}dx,$$

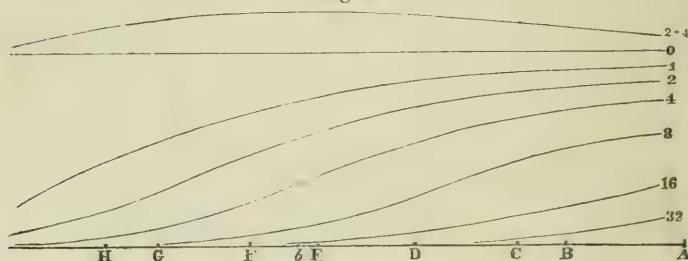
where k is a constant independent of λ . On integration,

$$I = I_0 e^{-k\lambda^{-4}x},$$

if I_0 correspond to $x=0$,—a law altogether similar to that of absorption, and showing how the light tends to become yellow and finally red as the thickness of the medium increases. Fig 2 shows a series of curves representing the composition of the originally white light after passing through thicknesses in the ratio of 1, 2, 4, 8, 16, 32. The reader will observe how little of the violet light remains when the red is still in nearly its original force. I cannot but think that this rapid diversion of the rays of short wave-length has a good deal to do with the absence of light of the highest refrangibility from the direct rays of the sun. For the line A at the extreme red and R near the upper limit of the photographic spectrum the wave-lengths are 7617 and 3108. The ratio of the fourth powers is about 36 : 1; so that, whatever

the fraction representing the transmission of A may be, its 36th power will give the transmission of R. To take an instance, if

Fig. 2.



·9 of the ray A gets through, only ·018 of R would be able to penetrate. For the rays of still higher refrangibility, which Professor Stokes found abundant in the electric light but missing in the solar rays, the fraction would be smaller still; but I am not aware of any measurements of smaller wave-length on which to found a calculation.

We have hitherto supposed that the light scattered by the finely divided matter reaches the eye without modification, and we have taken no account of any change in the composition of the primary light before diffraction. If x be the total length of the path of the ray through the turbid medium, we may express the quality of the light in terms of x ; for it makes no difference whether the lateral leakage takes place before diffraction or after. In fact

$$I \propto \lambda^{-4} e^{-k\lambda^{-4}x},$$

an expression which shows that I vanishes for very small as well as for very large values of λ , while for some definite value (say Λ) it rises to a maximum (I_0). Expressing I in terms of I_0 and Λ , we have

$$\frac{I}{I_0} = \frac{\Lambda^4}{\lambda^4} e^{1 - \frac{\Lambda^4}{\lambda^4}};$$

from which we may fall back on our original law by supposing Λ indefinitely small, and replacing $\Lambda^4 I_0$ by a finite constant. An approximate idea of the character of these lights may be obtained by subtracting the successive curves of fig. 2. Thus the difference of the curves marked 2 and 4 represents a light having its maximum brightness (of course relatively to the primary light) in the blue-green portion of the spectrum. I find by calculation that, if the maximum intensity be at b and be taken as unity, the intensities at G and C are given by the numbers ·713, ·710 respectively. The colour would be greenish; but whether

the green of the sky is to be accounted for in this way I am not able to say. Some, I believe, consider it to be entirely a contrast effect.

APPENDIX.

Within a space T , small in all its dimensions against λ , and situated at the origin of coordinates, let a force parallel to OZ , and, so far as it depends upon the time, expressed by a simple circular function, act on the medium. If ξ , η , ζ denote the displacements parallel to the axes at the point xyz , and

$$\delta = \frac{d\xi}{dx} + \frac{d\eta}{dy} + \frac{d\zeta}{dz},$$

$$\left. \begin{aligned} \frac{d^2\xi}{dt^2} &= b^2 \nabla^2 \xi + (a^2 - b^2) \frac{d\delta}{dx}, \\ \frac{d^2\eta}{dt^2} &= b^2 \nabla^2 \eta + (a^2 - b^2) \frac{d\delta}{dy}, \\ \frac{d^2\zeta}{dt^2} &= b^2 \nabla^2 \zeta + (a^2 - b^2) \frac{d\delta}{dz} + Z, \end{aligned} \right\} \quad \dots (A)^*$$

where ∇ stands for $\frac{d^2}{dx^2} + \frac{d^2}{dy^2} + \frac{d^2}{dz^2}$; a^2 and b^2 are constants

depending on the nature of the medium supposed to be isotropic. For the luminiferous æther, Green has shown that a is to be regarded as indefinitely great†.

To represent the periodic force, write for Z , $Z\epsilon^{int}$. Similar transformations will then apply to ξ , η , ζ , and δ ; so that on substitution in (A) and dividing out the common factor ϵ^{int} , there results

$$\left. \begin{aligned} (b^2 \nabla + n^2) \xi + (a^2 - b^2) \frac{d\delta}{dx} &= 0, \\ (b^2 \nabla + n^2) \eta + (a^2 - b^2) \frac{d\delta}{dy} &= 0, \\ (b^2 \nabla + n^2) \zeta + (a^2 - b^2) \frac{d\delta}{dz} &= -Z. \end{aligned} \right\} \quad \dots (B)$$

Writing

$$\frac{d\xi}{dy} - \frac{d\eta}{dx} = \varpi_3 \text{ \&c.},$$

* Thomson and Tait's 'Natural Philosophy,' p. 530.

† Camb. Phil. Trans vol. vii

we obtain from (B) by differentiation and subtraction,

$$\left. \begin{aligned} (b^2 \nabla + n^2) \varpi_3 &= 0, \\ (b^2 \nabla + n^2) \varpi_1 &= \frac{dZ}{dy}, \\ (b^2 \nabla + n^2) \varpi_2 &= -\frac{dZ}{dx}. \end{aligned} \right\} \dots \dots \dots (C)$$

$\varpi_1, \varpi_2, \varpi_3$ are the *rotations* of the elements of the medium round axes parallel to those of coordinates.

The disturbance which we are investigating is that caused and maintained by the force Z acting within the space T . Accordingly*

$$\begin{aligned} \varpi_3 &= 0, \\ \varpi_1 &= -\frac{1}{4\pi b^2} \iiint \frac{dZ}{dy} \frac{\epsilon^{\pm ikr}}{r} dx dy dz, \end{aligned}$$

r being the distance between the element $dx dy dz$ and the point where ϖ_1 is estimated, and

$$k = \frac{2\pi}{\lambda} = \frac{n}{b} \dots \dots \dots (D)$$

Since $\epsilon^{\pm ikr}$ will be finally multiplied by ϵ^{int} , and the disturbance which we are dealing with is propagated *outwards* from T , it is evident that the *lower* sign is to be employed. Now

$$\int \frac{\epsilon^{-ikr}}{r} \frac{dZ}{dy} dy = \left[Z \frac{\epsilon^{-ikr}}{r} \right] - \int Z \frac{d}{dy} \left(\frac{\epsilon^{-ikr}}{r} \right) dy,$$

of which the term within brackets vanishes, because the value of Z is only finite within the space T . Thus

$$\varpi_1 = \frac{1}{4\pi b^2} \iiint Z \frac{d}{dy} \left(\frac{\epsilon^{-ikr}}{r} \right) dx dy dz.$$

The factor $\frac{d}{dy} \left(\frac{\epsilon^{-ikr}}{r} \right)$ within the space T is sensibly constant, so that, if Z stand for the mean value of Z over the volume T ,

$$\begin{aligned} \varpi_1 &= \frac{TZ}{4\pi b^2} \frac{d}{dy} \left(\frac{\epsilon^{-ikr}}{r} \right), \\ \varpi_2 &= \frac{-TZ}{4\pi b^2} \frac{d}{dx} \left(\frac{\epsilon^{-ikr}}{r} \right), \\ x\varpi_1 + y\varpi_2 &\propto \left(x \frac{d}{dy} - y \frac{d}{dx} \right) \cdot \frac{\epsilon^{-ikr}}{r} = 0. \end{aligned}$$

* Helmholtz, *Crelle's Journal*, 1860.

And if $R = \sqrt{x^2 + y^2}$,

$$\begin{aligned}\varpi &= \frac{x\varpi_2 - y\varpi_1}{R} = \frac{-TZ}{4\pi b^2 R} \left(x \frac{d}{dx} + y \frac{d}{dy} \right) \cdot \frac{\epsilon^{-ikr}}{r} \\ &= \frac{-TZ}{4\pi b^2} \frac{d}{dR} \cdot \frac{\epsilon^{-ikr}}{r} = \frac{TZ \sin \alpha}{4\pi b^2} \frac{d}{dr} \cdot \frac{\epsilon^{-ikr}}{r},\end{aligned}$$

where α denotes the angle between r and z .

The resultant rotation at any point is thus about an axis perpendicular to the plane passing through the point and the axis of Z ; and its magnitude is given by ϖ . In differentiating $\frac{\epsilon^{-ikr}}{r}$ with respect to r , we may neglect the term divided by r^2 as altogether insensible, kr being an exceedingly great quantity at any ordinary distance from the origin of disturbance. Thus

$$\varpi = \frac{-ik \cdot TZ \sin \alpha}{4\pi b^2} \cdot \frac{\epsilon^{-ikr}}{r}, \quad \dots \quad (E)$$

which completely determines the rotation at any point. For a given disturbance it is seen to be everywhere about an axis perpendicular to r and the direction of the force, and in magnitude dependent only on the angle between these two directions (α) and on the distance (r).

The intensity of the light, however, is more usually expressed in terms of the actual displacement in the plane of the wave. In order to find the connexion between the two quantities, it will be more convenient to suppose the scattered ray parallel to x , and that the force F (for Z is no longer appropriate) acts in the plane of zx at an angle α with Ox . ϖ becomes identical with ϖ_2 ; that is, with $\frac{d\zeta}{dx}$; for ξ as well as η is zero; so that

$$\zeta = \int \varpi dr = \frac{TF \sin \alpha}{4\pi b^2} \cdot \frac{\epsilon^{-ikr}}{r}.$$

Restoring the factor ϵ^{int} , we have

$$\zeta = \frac{TF \sin \alpha}{4\pi b^2} \cdot \frac{\epsilon^{i(nt-kr)}}{r};$$

or throwing away the imaginary part,

$$\zeta = \frac{TF \sin \alpha}{4\pi b^2 r} \cdot \cos \frac{2\pi}{\lambda} (bt - r). \quad \dots \quad (F)$$

This corresponds to a total accelerating force equal to

$$FT \cos \frac{2\pi}{\lambda} bt;$$

a result which agrees with that of Professor Stokes's more complete investigation, with the exception of a slight difference of notation.

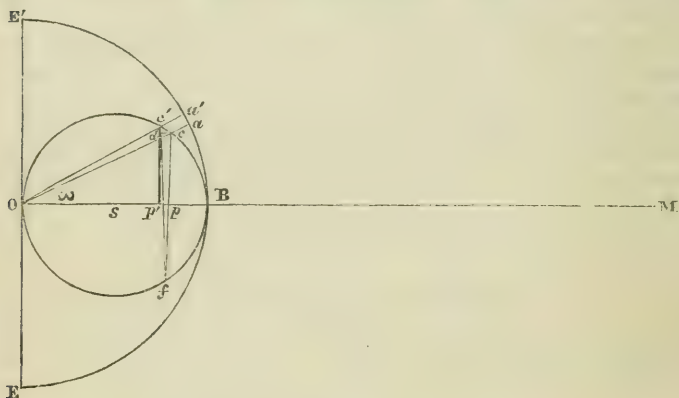
Terling Place, Witham,
December 30, 1870.

[To be continued.]

XVI. *Addendum to a Paper on the Theory of the Tides. By the Rev. T. K. ABBOTT, Fellow and Tutor of Trinity College, Dublin*.*

IN a former Number of this Magazine (Jan. 1870) I gave elementary proofs of the principal theorems in the Theory of the Tides. In order to complete the elementary treatment of the subject, it is desirable to give simple constructions for the disturbing force, the velocity of the current, and the height of the tide. The two latter I propose to give in the present paper; it would be superfluous to occupy your space with the former.

It is in fact readily shown by elementary geometry that in the figure †, where EBE' represents the hemisphere under the



moon, and a circle $Oc'Bf$ is described round the radius directed to the moon, the tangential disturbing force at a is proportional to the perpendicular cp . If aa' be the space passed over in the

* Communicated by the Author.

† By inadvertence the direction of the motion in this figure is the reverse of that in the figure in the former paper.

rotation of the earth in one second, the force acting on the water may be supposed unchanged while it passes from a to a' ; and its effect during that interval (*i. e.* in this quadrant the retardation) will also be proportional to cp or its double cf , and to the time, that is, to aa' , or the angle at O , aOa' . Now the angle at O = the angle at f , being in the same circle; and this angle multiplied by cf = the small perpendicular cd , or pp' , which is parallel and equal to it. Therefore the whole retardation since leaving B is proportional to the sum of all the abscissæ pp' —that is, to Bp' . This represents the defect from the greatest eastward velocity; and after passing its mean value at the middle point s it represents a velocity which, relatively to the earth, is westerly. The velocity of the current relatively to the earth is represented by ps .

Now if at any point in the supposed canal a thin section be taken, the quantity of water entering this section in a given time is proportional to the depth and the velocity. If the water flows in a little more rapidly than it flows out, it is clear that the increase in the quantity contained in the section, and therefore the increase in depth, will be proportional to the difference between these two velocities and to the whole depth. This holds as long as the change is small compared with the whole depth. If this be supposed uniform throughout the canal, the increase in it (that is, in the height of the tide) at a' is therefore proportional to the retardation; and since the tide began to rise at B , where the velocity began to diminish, it follows that Bp' is also proportional to the height of the tide at a' above its lowest point.

It is easy to deduce from this construction the corresponding formulæ. For calling OB, r , we have $Bp = r(1 - \cos^2 \omega)$. But $ps = \frac{1}{2}r - Bp = \frac{1}{2}r(2 \cos^2 \omega - 1) = \frac{1}{2}r \cos 2\omega$. And since sB is proportional to the mean height, the defect from this height is proportional to ps , and therefore to $\cos 2\omega$.

I have one remark to add with respect to the former paper. It was observed there that, in proving that without friction there would be low water under the moon, it was assumed "that the ocean is carried round by the earth in its rotation." It appears to be *à priori* an admissible supposition that this is not the case, but that the ocean is in a state of equilibrium under the moon's action while the earth rotates. But this would obviously imply an apparent movement of the whole body of water, relatively to the earth, with a velocity equal and opposite to that of the earth's rotation, *i. e.* at the equator there would be an apparent current of about 1000 miles per hour. As this does not correspond to the fact, the supposition is practically inadmissible. But when friction is considered, it appears theoretically inadmissible also. For in this case friction would be continually acting in the same

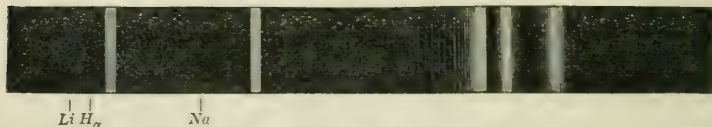
direction, and its effect would be to make the eastward forces preponderate; so that although the ocean should be supposed to be at rest at the first, it would ultimately be dragged round by the earth.

Letterkenny, Ireland.

XVII. On the Spectrum of the Aurora borealis.

By F. ZÖLLNER*.

ON the 25th instant I observed, by means of a Browning's miniature-spectroscope, the spectrum of the aurora borealis as shown in the annexed engraving. To obtain sufficient brightness, the slit was opened rather wide.



With an alcohol-flame, the wick of which was impregnated with sodium- and lithium-salts, the lines of lithium and sodium were at the same time produced; and their position was used for the approximate determination of the lines of the aurora borealis. The line in the green part of the spectrum is, in all probability, the one first observed and approximately determined by Ångström†. On the contrary, the above-represented red line has not, to my knowledge, till now been observed. Certainly this line occurs with sufficient intensity only in those parts of the sky which even to the naked eye appear intensely reddened. But the green line also was always present in these spots, and, indeed, so intense that very seldom did the red line produce an equally strong optical impression. In the blue portion of the spectrum there only sometimes occurred faint band-like streaks, among which a broad dark band on a brighter ground was the most striking.

Dr. Vogel had the goodness to send me the drawing of the spectrum of an aurora observed by him in conjunction with Dr. Lohse, about the same time, at the observatory of Chamberlain von Bülow at Bothcamp, near Kiel. It showed the bright lines in the green part, and flutings on both sides very quickly diminishing in intensity, similar to the light-maximum of a diffraction-spectrum; but the red line was not present.

It was not till after the aurora had vanished that I was able

* Translated from the *Berichte der Kön. Sächs. Gesellschaft der Wissenschaften, math.-phys. Classe*, Oct. 31, 1870.

† Compare Ångström's *Recherches sur le Spectre solaire*, p. 41. Berlin, 1869.

to observe with *the same* spectroscope the spectra of hydrogen, nitrogen, oxygen, and carbonic acid in Geissler's tubes. Yet I believe that I have ascertained, by simultaneous observation of the sodium and lithium spectra, that the red aurora-line does not agree with any maximum of brightness in the spectra of the four gases investigated. It is more refrangible than the red hydrogen-line C; and its situation would be nearest to the place of the group of dark atmospheric lines α , between C and D in the solar spectrum, to which corresponds a mean wave-length of 0.0006279 millim. It may therefore be maintained that the spectrum of the aurora borealis corresponds, in its principal lines, with none of those of known terrestrial substances hitherto observed. On the contrary, the observations of Winlock and Young* make it very probable that three green lines of the spectrum of the protuberances observed during a total eclipse of the sun coincide with three lines in the spectrum of the corona and in that of the aurora borealis. According to the measurements hitherto made, the most refrangible of these lines coincides with one which, according to Kirchhoff's scale, is numbered 1474, and, according to Ångström, has a wave-length of 0.0005323 millim. Kirchhoff and Ångström agree in designating this line as one belonging to the spectrum of iron.

All these circumstances seem only to increase the difficulties opposed to a satisfactory explanation of the aurora borealis; so that Ångström, in the work above cited, declares the hitherto assumed analogy between the phenomena of the light of the aurora and those presented by the passage of electricity through rarefied air to be disproved by the non-coincidence of the aurora-spectrum with any known spectrum of the atmospheric gases†. I nevertheless believe that, by the following considerations, I shall be able to make the assumption very probable, that, if the luminous developments exhibited in the aurora are indeed, after the analogy of the rarefied gases brought to incandescence in air-exhausted spaces, of an *electrical* nature, they must belong to so low a temperature that it is impossible *at the same temperature* to observe the spectra of incandescent gases in Geissler's tubes. This would render possible and, as very simple, also probable the following explanation:—*That the spectrum of the aurora borealis does not correspond with any known spectrum of the atmospheric gases is only because, though a spectrum of our atmosphere,*

* Silliman's American Journal, Nos. 142 and 143, 1869.

† *Recherches sur le Spectre solaire*, p. 41:—"Moreover, the two phenomena (the aurora borealis and terrestrial magnetism) being so intimately connected that the appearance of the former is always accompanied by perturbations acting on the magnetic needle, it was possible to suppose that the aurora borealis was only a play of electricity, analogous to that produced by the rarefied air in the electric egg; *this, however, is not the case.*"

it is one of another order, and one which we cannot yet produce artificially.

The considerations in question result from the theorem developed in the preceding memoir*, of the equivalence of thickness and density of the radiant layers in the conditions of density prevailing in the higher regions of our atmosphere. Let us denote by A_λ and E_λ the value of the absorptive and the emissive power, referred to unity of thickness and density at a determined temperature, for the wave-length λ ; further, let m and σ be the thickness and density of the luminous layer of gas; we thus have for the brightness E of the part of the spectrum belonging to λ , according to the theorem mentioned, the expression

$$E = [1 - (1 - A_\lambda)^{m\sigma}] \frac{E_\lambda}{A_\lambda}.$$

It will be seen that, for a given gas and a given temperature, this expression depends only on the value of the product of $m\sigma$.

Let us conceive, then, for example, a Geissler's tube filled with rarefied atmospheric air; and let us take the diameter of the central channel at 1 millim. as unit for the thickness m , and as unit for σ that density of the included air which corresponds to a temperature of 0° C. and a pressure of 1 millim. of mercury. If now by means of an induction-apparatus the air in the tube is rendered incandescent, at constant temperature the spectrum of the incandescent layer of gas would remain qualitatively and quantitatively unaltered, if the thickness of the luminous layer (consequently, in the case considered, the diameter of the channel) were increased from 1 millim. to 1000 millims., but to compensate for this the pressure of the included gas were diminished from 1 millim. to $\frac{1}{1000}$ of a millim. Wüllner, in his investigations "On the Spectra of Gases in Geissler's Tubes," found that the current of a smaller Rühmkorff's apparatus, in the cases of nitrogen and oxygen, could not overcome the resistance until the pressure in the tubes had been reduced by exhausting to 94 and 64 millims. respectively. But although at these pressures the gases became permanently luminous, yet the intensity of the light was still too small for spectroscopic investigation. With nitrogen the requisite brightness was first obtained when the pressure was reduced to 46 millims.; with oxygen, to 28-30 millims†.

I will hence assume that, with the apparatus used by Wüllner, at about 50 millims. pressure the induction current passes through

* "Ueber den Einfluss der Dichtigkeit und Temperatur auf die Spectra glühender Gase" (Ber. Kön. Sachs. Ges. d. Wissensch. Oct. 31, 1870, p. 233), a translation of which will appear in the Phil. Mag.

† Pogg. Ann. vol. cxxxv. pp. 516 & 524.

a Geissler's tube filled with atmospheric air, and with a thickness of the radiant layer of 1 millim. a sufficient amount of light is developed for the observation of the atmospheric spectrum. But if now this thickness of the luminous layer be compared with the thicknesses present in the aurora borealis, the latter must evidently be taken as incomparably greater, and, especially at spots far from the zenith, may be estimated at miles*. But if we even assume for the thickness of such a layer only 1 kilometre, this, at the same temperature as that in the Geissler's tube in the present example, could have only the millionth part of the density of the air contained in the tube, and so only 0.00005 millim. pressure at 0° C., if the spectrum of the incandescent gas in the tube were to correspond perfectly with that of the aurora borealis†. Here, then, the question arises, whether it is allowable to assume such low rates of pressure in those regions of our atmosphere in which the aurora borealis is developed.

As regards the height the accounts of various auroræ boreales are very divergent. Thus, not to mention earlier accounts, Hansteen‡ finds for the aurora of January 7, 1831, the height of 26 [German] geographical miles, by combining the measurements of the apparent height of the dark segment at Berlin and at Christiansand in Norway. On the contrary, the observations of Farquharson§ make it probable that auroræ boreales in general have a much less altitude, and sometimes descend quite to the region of the clouds.

The latter view is corroborated by many more recent observations, especially in the polar regions, made by Parry, Wrangel, Franklin, Hood, Richardson, and others; so that the height of from 10 to 20 geographical miles, assumed in the following calculations as that of the lower parts of the aurora borealis, may be presumed to be at all events rather too great than too small. If, assuming a uniform temperature everywhere of 0° and the normal condition of the barometer at the surface of the earth, we calculate the atmospheric pressure at those heights, we shall find

* This may offer a very simple explanation of the fact that, in general, with increasing distance from the zenith the brightness of the aurora borealis increases, as far as the boundary of the dark segment.

† In all these considerations, it is obvious that the apparent magnitude of the luminous layer is always assumed to be sufficient to cause the variation of distance of the latter to have no effect on the brightness of the spectrum. In like manner the influence of absorption in the deeper and non-luminous layers of the atmosphere is neglected.

‡ Pogg. *Ann.* vol. xxii. (1831).

§ "On a definite Arrangement and Order of the Appearance and Progress of Aurora borealis, and its Height above the surface of the earth," *Phil. Trans.* 1829; and "On a remarkable Appearance of the Aurora borealis below the Clouds," *ibid.* 1842.

for it at the height of 10 geographical miles 0·078 millim., and at the height of 20 miles 0·00001 millim.

To a layer 1 metre thick of the incandescent air of the aurora borealis, then, at the height of 10 miles, a pressure of 78 milims. in the Geissler's tube would be equivalent, and at the height of 20 miles a pressure of 0·01 millim., in order to produce at the same temperature a spectrum just as bright as that of the aurora borealis.

But since, as before remarked, the thickness of the luminous layers in the aurora is most probably to be estimated as amounting to, not *metres*, but *kilometres*, we should obtain so high a value for the pressure in the Geissler's tube equivalent to the thickness even at the height of 10 miles (namely, 78 metres of mercury for a layer 1 kilometre thick), that the quantity of electricity produced by even the largest induction-apparatus would not possess sufficient tension to overcome the resistance of the air compressed to such a degree. And even if the tension were sufficient, the temperature produced by the discharge would be so high that the spectrum could only be brilliant and continuous, and hence not comparable with that of the aurora borealis.

From this it is evident that the quantity of incandescent gas particles in a Geissler's tube, in comparison with that in the aurora borealis, is probably extraordinarily minute. But since, notwithstanding this, the spectrum of a gas rendered incandescent in such a tube by electricity must *at least* possess the brightness of the aurora-spectrum in order, even with the best apparatus as regards intensity of light, to afford a spectroscopic analysis, it follows that the emissive power of the incandescent gas particles in the tube must be immensely greater than that of those in the aurora borealis. Such a difference of emissive power, however, can only be effected by difference of temperature.

If, then, the light developed in the aurora borealis arises from incandescent gas particles of our atmosphere, the temperature at which this takes place must be very much lower than that necessary to render the same gases incandescent in Geissler's tubes.

Still, according to Kirchhoff's theorem, the temperature may not be lower than that of a perfectly black body in a state of incandescence, the continuous spectrum of which, in the places corresponding to the aurora-spectrum, is of equal brightness with this.

From these considerations it results that all the gas-spectra, of various orders, which we can artificially produce can, generally speaking, only belong to *high* temperatures; for the relatively great brightness of so small a quantity of the incandescent particles shows that there must be a great emission of light from

each individual particle, and this can only be effected by a high temperature.

If, on the other hand, we observe very simple and faintly luminous spectra where, in spite of the marvellously tenuous distribution of the substance (as in the aurora borealis, the corona, the zodiacal light, and nebulae), considering the enormous thickness of the radiant layers it must be admitted that a great number of luminous particles are effective, the temperature of the gases here incandescent may in general be relatively low. These considerations satisfactorily corroborate the conjecture of Lockyer*, that the simple spectra of nebulae do not belong to high, but to relatively low temperatures.

Remark.—Probably the admissibility of the comparison above instituted between the aurora borealis and a Geissler's tube might be doubted, because in the latter the gas, on being heated, cannot expand, as in the aurora, and hence its density remains constant. Yet, as regards the above considerations, but very little importance can be ascribed to this circumstance, because the diminution of the density is only a secondary effect of the increase of temperature, and occurs relatively much later than the optical effect of the incandescence; and even if it were taken into account, it would necessitate only unimportant alterations in the example treated. On the other hand, I consider it probable that at least a part of the remarkable phenomena of motion observed in the aurora borealis are to be referred to the disturbances of equilibrium arising from such important differences of temperature in the extremely rarefied strata of our atmosphere.

XVIII. On *Athermogenic Condensation*.

By the Rev. J. M. HEATH†.

THE proposition which I understand is good thermodynamics, though to my mind very questionable mechanics, may be thus stated. If P and Q are the forces tending respectively to contract and expand the volume V of a unit of gas, and if $(P - Q)\delta V = 0$, and if actual contraction of V does take place by the uniform descent of the piston through the space δV , then the internal energy of the gas will be augmented by the energy due to the action of P through the space δV .

Those who accept this proposition do so in the belief that it is as true in mechanics (that is, when the energy spoken of is regarded as molecular motion) as it is in thermodynamics, where such energy is identified with heat. And they admit, as a mechanical prin-

* Proc. Roy. Soc. 1869, No. 112.

† Communicated by the Author.

ciple, that when $(P - Q)\delta x = 0$, P can generate no motion, but with the remarkable reservation that this is to be understood only of motion in the point or body to which P and Q are actually applied: but it seems to be held that, under certain circumstances at least, P *may* augment the motions of other bodies to which it is not applied.

In order to maintain the equality of the forces acting upon the piston during its descent, and therefore the uniformity of its motion, it is necessary, since Q increases during the contraction of V, to augment P also by a quantity constantly equal to the augmentation of Q. Mr. Rankine finds $M \frac{du}{dt}$,

where M is the mass of the piston, and u its velocity, for the value of this increment of P. And he finds the expression $2m \cdot u \cdot (u + v)$ for the increment of the internal energy of the gas caused by the descent of the piston through the space udt . He states as his final result that $Pudt + Mudu = 2mu \cdot (u + v)dt$, which he interprets in words to mean that the internal energy of the gas will be increased by a quantity represented by *the work done by P + the energy lost by the piston through retardation*.

I cannot admit the correctness of the equation in which Mr. Rankine has expressed the final result of this his corrected investigation. If I have myself rightly apprehended the argument he has followed, the result should have been symbolically expressed as follows.

Before the motion the equality of action and reaction gives

$$P \cdot udt = Qudt,$$

during and after the motion we have

$$P \cdot udt + Mudu = Qudt + 2mu(u + v)dt;$$

therefore

$$Mudu = 2mu(u + v)dt.$$

And this result, being independent of both P and Q, is conclusive *against* the proposition it was advanced to support, viz. that the energy generated was to be proportional to the whole action of P through udt or to $Pudt$.

But, on the other hand, I must admit that although it supports the principle I have been contending for, that when new energy is generated it is due to the difference between P and Q and not to the entire action of either of them, yet so far as it seems to show that some new energy is generated in contraction, even while P and Q are always in equilibrium, I am bound to remove this difficulty, or else to admit that a contrary opinion to my own has been, at least to a certain extent, established.

The energy of repulsion by which the piston repels the par-

ticles impinging upon it is, when the piston is in motion, $\frac{u+v}{v}$ times as great as it was when the piston was at rest. It will therefore create $\frac{u+v}{v}$ times as much momentum in m particles in the one case as in the other. The momentum of m particles after reflection from the piston at rest was mv . It is therefore $\frac{u+v}{v} \cdot mv$ or $m \cdot \overline{u+v}$ when reflected by the moving piston. But the number of particles which impinge upon the piston in the time dt is in the second case $\frac{u+v}{v}$ times what it was in the first, and therefore the average velocity which these must receive in order to acquire the same aggregate momentum will be

$$\frac{m \cdot (u+v)}{m \cdot \frac{u+v}{v}} = v.$$

It thus appears that the moving piston reflects $\frac{u+v}{v}$ times as many particles as the stationary one in the same time, but without altering their velocity. But the *pressure* of the gas depends upon the *frequency of these reflections when their velocity is unaltered*; and the energy of the gas depends upon *the alteration of the velocity*; so that when the motion of the piston is uniform, as in the present case, the increment of the condensing energy all goes into pressure, and none of it into motion. When the motion of the piston is accelerated, the excess of energy which causes the acceleration then increases the *vis viva*.

XIX. On a small Universal Stellar Spectroscope.

By M. SIGMUND MERZ.

[With a Plate.]

To Dr. Francis, F.L.S. &c.

SIR,

Munich, November, 1870.

IN Carl's *Repertorium der Physik*, vol. vi. p. 273, there is contained the description of a small universal star-spectroscope constructed by me, accompanied by an explanatory Plate. As, however, since that article appeared this compendious little instrument has been improved in various essential particulars, at the suggestion of a friend I make bold to forward to you a short description thereof, and also a corrected sketch of its arrangement.

The instrument consists of two principal parts, or, in other

Phil. Mag. S. 4. Vol. 41. No 271. Feb. 1871. K

words, of two spectroscopes, each of which may be used singly, and which are in part complementary to each other. The one part, which is a simple direct-vision spectroscope, is provided with a prism of novel construction. This prism consists of five elements, two of which are of flint glass, each having a refracting-angle of 84° ; the other three prisms are of crown glass; one of them has also a refracting-angle of 84° , the other two of 87° , as shown in Plate II. fig. 4.

This portion of the apparatus is furnished with a positive eyepiece with an equivalent focal length of 1 inch, and it is also provided with a cylindrical lens of 1 inch radius. The arrangement of this portion of the apparatus is shown in fig. 1.

The compound spectroscope, which is represented in fig. 2, is provided with a similar spectral prism. It has a tube furnished with a slit, and a collimator, and likewise an observing-telescope. The object-glasses of the latter and of the collimator are of the same focal length, namely 4 inches; their aperture is $\frac{2}{3}$ of an inch.

The observing-telescope carries, moreover, a needle-micrometer with an eyepiece of $\frac{1}{2}$ -inch focal length. It is also provided with two red sun-glasses, which admit of being used either singly or together. This compound spectroscope is moreover furnished with a small divided position-circle situated at B.

The spectral prism can be unscrewed from the simple apparatus (fig. 1) and inserted at A, between the observing-telescope and the spectral prism of the compound apparatus (fig. 2), whereby the dispersion of the spectroscope is doubled, and the entire instrument rendered available for the observation of the solar protuberances.

With the view of such observations, a peculiar construction has been given to the slit (fig. 3), by means of which, through the medium of a screw, its two steel jaws can be simultaneously moved away from the centre and opened to the distance of nearly a quarter of an inch. The slit is further fitted with an arrangement for throwing aside the illuminating prism that is employed in comparing the spectra of flames. This latter movement is effected by means of a small lever, to the long arm of which the prism in question is attached, and the short arm of which is inserted into an aperture in the rotating ring R. When such a motion has been given to this ring that the aperture at which the light enters (*a*) is closed, the prism no longer remains opposite the slit, so that the cone of rays derived from the refractor has then free passage through the full width of the jaws.

The lateral movement near K on the observing-telescope enables any separate colour of the spectrum to be brought into position, while a similar movement at K' near the slit serves for altering the angle of incidence of the rays, thus bringing into

Fig. 2.

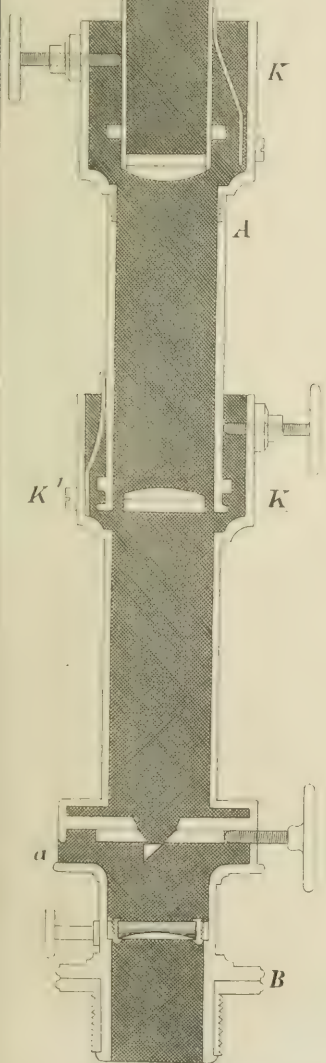


Fig. 1.

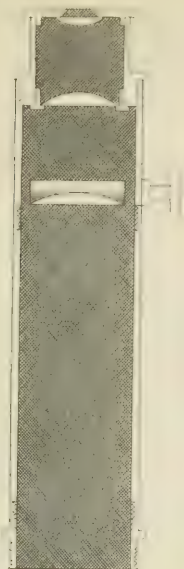


Fig. 4.

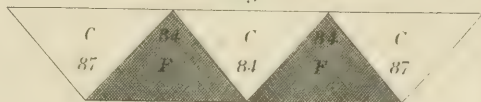
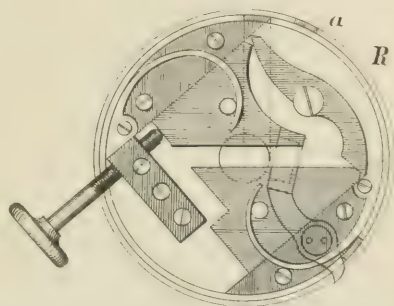


Fig. 3.



the field of view the extreme portions of the spectrum, even when extended by the employment of the two systems of prisms.

With respect to the rectification of the instrument, it is only necessary to observe that all the parts that unscrew are provided with suitable marks; and, speaking generally, the apparatus may be looked upon as in adjustment when the so-called Fraunhofer lines are situated at right angles to the spectrum.

The cylindrical lens, it should be remarked, is to be employed only in stellar observations; in other cases it should be removed. It admits of being turned round in position 90° , and can thus be placed either normal or parallel to the slit. If, for instance, the cylindrical lens is situated within the focus of the refractor and with its axis normal to the direction of the slit, it will elongate the little circular disk of a star parallel to the slit, or, in other words, it will form a luminous line coinciding with the direction of the slit, whereas in the opposite case the caustic of the cylindrical lens will lie crosswise to the slit. It should therefore be placed normal to the slit when observing in converging light, or when placed within the focus of the refractor; while, on the contrary, it has to be set parallel to the slit when observing beyond the focus, or in diverging light, in which case the caustic of the cylindrical lens coincides with the slit as before. And since the rotation of the cylindrical lens causes a change of direction in its caustic to the amount of 90° , this motion admits of the apparatus being always brought into the proper position, whether the cylindrical lens be in the converging or in the diverging cone of rays of the refractor.

Bearing in mind that the little observing-telescope and the collimator are of equal focal length, it is easy to calculate the spectroscopic amplification of the refractor employed. The amplification is equal to twice that of the focal length of the refractor, expressed in French inches, when the half-inch eyepiece is used, and is equal to the simple focal length when, instead of the half-inch, the 1-inch eyepiece is employed. Were the focal length of the observing-telescope and of the collimator not equal, the amplification would increase as the focal length of the collimator diminished, and *vice versa*.

With reference to the efficiency of the instrument here described, I beg leave, in conclusion, to quote the following passage, translated from a letter addressed to me by Dr. Schellen of Cologne, on the 27th of September last. The writer therein says, "after duly adjusting your refractor of 34 lines aperture, in the possession of Herr von Camphausen, I was not only enabled to see at once brightly and with great distinctness the lines $H\alpha$, $H\beta$, and $D3$, but also, on widening the aperture of the slit, to observe at different places on the sun's limb various solar protuberances."

The price of the complete apparatus, with the improvements above described, and packed in an elegant case, is 240 Bavarian florins, which are equal to £20.

I am, Sir,

Yours respectfully,

SIGMUND MERZ.

XX. Estimation of Zinc on Galvanized Iron.

By THOMAS T. P. BRUCE WARREN*.

MANUFACTURED articles of iron, when coated with zinc, are said to be "galvanized." The word galvanized was probably suggested by the fact that iron covered with zinc was, in its earlier introduction, produced by means of galvanic action.

Iron in the form of wire is galvanized by drawing through a bath of molten zinc, and in other forms (as sheets &c.) by simple immersion or dipping. The surface of the iron being rendered chemically clean immediately before coming into contact with the zinc, a portion of the zinc combines with the iron at its surface and forms a thin layer of alloy, to which a further quantity of zinc readily adheres. The alloy of iron and zinc, when freed from the uncombined zinc, is capable of taking up a definite quantity of mercury, which, when carefully heated, is again given off without any apparent alteration in the alloy.

The following process, which is based upon these considerations, is recommended more especially to the notice of telegraph engineers for estimating quantitatively the amount of zinc on galvanized iron wires.

The wire when weighed is carefully cleaned with dilute sulphuric acid and immersed in mercury for a few hours, or until all the zinc which is capable of being dissolved is removed. The crystalline amalgam, which after a short time forms on its surface, should be wiped off and the wire left undisturbed at the bottom of the mercury for four or eight hours.

The adhering particles of mercury being removed, the wire is well rubbed with a chamois leather and weighed. The loss† will not only show the extra quantity of zinc on the wire beyond what is actually required for producing a chemically galvanized surface, but will expose those parts of the wire to which the zinc has not combined and which would in a short time scale off. The wire, if properly galvanized, should be uniformly coated with amalgam, and present a bright silvery appearance when rubbed.

This wire is now heated so as to expel the mercury, and again

* Communicated by the Author.

† To this must be added the weight of mercury retained by the alloy.

weighed when cooled. From this loss we may obtain with tolerable exactness the amount of zinc which remains, and which immersion in mercury will not remove.

This quantity of zinc appears to be combined with the iron, as it does not volatilize, and becomes very imperfectly oxidized when exposed for some time to a red heat under access to air.

The existence of the zinc, after this treatment, may be shown by the readiness with which the iron wire again takes up mercury, and admits of being polished. The smallest quantity of zinc remaining on the wire becomes immediately visible by its lustrous appearance when rubbed after immersion in mercury: when the whole of the zinc is removed, the wire ceases to give a silvery appearance. The removal of the zinc immediately in contact with the iron can only be effected by the oxidation of the iron and zinc together.

Repeated experiments prove that this quantity of zinc, which is combined with the iron, can be readily ascertained from the quantity of mercury which sublimes when the wire is held in a non-oxidizing flame. The mean of several observations shows that the mercury is taken up in the proportion of eleven parts to seven parts of zinc. The difference between the extremes from which this mean is taken is $\cdot 025$ in eleven parts mercury. The proportion of zinc has been verified by analysis in the humid way.

The following experiment will more clearly illustrate the operation of the test:—

Weight of galvanized wire . . . = 50 grains.

Weight after immersion in mercury = 46 „

Weight after expelling mercury . . = 45 „

This will give 5 grains for the weight of zinc removed by immersion in mercury.

Weight of mercury taken up by alloy = 1 grain,

which will represent $0\cdot 46$ grain of zinc combined with the iron. Consequently 50 grains galvanized iron wire contains $5\cdot 46$ grains zinc, of which only $0\cdot 46$ is in chemical combination.

In all the specimens examined, the irregularity in the thickness of the zinc was so great that no reliance can be attached to its estimated value.

Tamworth House,
Mitcham Common.

XXI. *Researches on the Specific Heats, Densities, and Expansions of some Liquids.* By C. MARIGNAC*.

PART I. SPECIFIC HEATS.

WHEN narrating, in a previous memoir†, some preliminary researches upon the part played by water in saline double decompositions, and its influence on the thermal effects which accompany them, I noticed some apparently very singular facts, but could not yet regard them as certain, since in my calculations I had not taken account of the specific heats of the solutions employed.

Intending subsequently to resume their examination by more rigorous methods, I undertook a series of researches on the specific heats of the solutions. I was not then aware that M. Thomsen had been for some time engaged in a work of the same kind and had nearly brought it to a conclusion. He has just announced that the results of his observations (of which he has already given a summary‡) will shortly be published.

Although this publication almost renders unnecessary the continuation of my work, and mine are still far from being so comprehensive as M. Thomsen's researches, I wish to state the results at which I had already arrived; for several of the compounds on which I have been occupied have been likewise the subject of his experiments, and it will not be uninteresting to compare results which, obtained by very different methods, will show, if one may judge from those he has already published, a very satisfactory accordance. I have also examined some solutions to which, perhaps, he has not turned his attention, and which will lead me to add some general observations to those which he has already presented.

For the determination of the specific heats of aqueous solutions, I have used one of the most simple and well-known methods: it consists in measuring the rise of temperature produced in an ascertained weight of liquid by the introduction of a heated body.

Two objections have been made to this method, impugning its accuracy.

The first is founded on the difficulty of knowing exactly the temperature of the body which is immersed in the liquid, or rather (for I shall show, further on, that in operating as I have

* Translated from the *Archives des Sciences Physiques et Naturelles* for November 1870.

† *Arch. des Sci. Phys. et Nat.* vol. xxxvi. p. 319.

‡ *Berichte der Deutschen chemischen Gesellschaft zu Berlin*, 1870, p. 716; and *Archives*, vol. xxxix. p. 153.

done, this knowledge is of little importance) of being perfectly sure that it has always the same temperature.

To remove this difficulty, I use as source of heat a mercurial thermometer with a large reservoir. As it is important that the heat be transmitted as rapidly as possible, I have had thermometers constructed the reservoir of which is formed of a tube of 9 millims. internal diameter, and about 50 centims. long, spirally coiled, so as to admit of being entirely immersed and agitated in a cylindrical vessel containing from 150 to 250 cubic centimetres of liquid. The value in water of these thermometers varied from 15 to 21 grammes. The transmission of the heat was sufficiently rapid for the maximum to be reached in 30 seconds. The stems of these thermometers were graduated in divisions of about a millimetre, of equal capacity, and corresponding to about $\frac{1}{10}$ of a degree.

Before the experiment, the thermometer is heated, in a stove, to a few degrees above the temperature at which it is intended to be immersed in the liquid. It is then taken out and gradually approached to the vessel, and the immersion takes place the moment the mercury arrives at the point determined. I do not think that the error which one may commit in operating thus will amount to a quarter of a division ($\frac{1}{40}$ of a degree); and as the thermometer was immersed at a temperature about 40° above that of the liquid of which the heating was to be measured, it is evident that the possible error from this source could not exceed $\frac{1}{1600}$.

The second and more serious objection is the inexactness of all the methods proposed for correcting the errors produced by the radiation of the vessel which serves as calorimeter. But there is a very simple means of rendering correction needless. Instead of comparing, as has generally been done, the variations of temperature produced by the same source of heat in arbitrary weights of water and the liquid whose specific heat we wish to measure, it is sufficient to reverse the problem, and determine the relative weights of water and of the liquid which undergo the same alteration of temperature by the addition of the same quantity of heat. In this way, and especially if the experiments are made immediately after one another, in the same atmospheric conditions, the influence of all exterior causes acts in precisely the same manner in each case, and consequently involves no perturbation.

Besides, it will be understood that here absolute identity of temperature is not requisite. Provided that, in the two experiments compared, the temperatures differ by only a few hundredths of a degree (which it is very easy to ensure after a single preliminary experiment), all correction may be dispensed with.

The variation of temperature produced in the liquid by the immersion and agitation of the heated thermometer was measured by a very sensible thermometer, the divisions on which corresponded to $\frac{1}{30}$ of a degree, and which was read by means of a horizontal telescope.

The saline solutions were contained in a silvered brass cylindrical vessel, of which the value in water had been determined by previous experiments, and found to be 13 grammes, including the thermometer. However, even a notable error in this value would only involve a scarcely perceptible one in the result of the experiments, since it would affect equally the specific heats of water and the solution compared with it.

For strongly acid liquids I used a vessel of very light glass, of which the equivalent in water was 14.9 grammes.

To avoid as much as possible accidental and variable external influences, these vessels were suspended by their spread upper brim, by means of a tin plate perforated with a circular hole, in the middle of a large cylinder of the same metal, itself immersed in water at the surrounding temperature.

For the experiments to be perfectly comparable, it is necessary to wait, before commencing them, till the temperature of the liquid in the calorimeter is quite stationary under the influence of the external radiation and of the superficial evaporation. This makes the first experiment of a day rather long; but it is easy afterwards, by following the course of an exterior thermometer, to bring the liquid immediately to such a temperature that it will not vary sensibly during a much longer time than the duration of a determination.

When about to commence this investigation, I thought it would be sufficient for me to determine by a series of trials made upon water an empiric relation expressing, for each difference between the temperature of the heated thermometer and the temperature of the bath, the ratio of the quantities of heat given up by the first and gained by the second. But I had to abandon this idea, and to compare directly the experiments made on each solution with similar experiments, made immediately before and after, on water.

In fact, I have remarked that, while all the determinations made in one and the same day, or sometimes in two consecutive days when the atmospheric conditions have not changed, are in general very accordant, it is not quite so satisfactory when experiments made at periods somewhat more distant are compared, even when the conditions of temperature are the same. This may be due either to atmospheric influences other than the temperature (for example, the hygrometric state of the air), or to the fact that, at periods a little distant, one does not preserve

the same uniformity in the movements necessary for immersing and agitating the heated thermometer in the bath.

As each experiment could be made very rapidly, the determination of the specific heat of each solution was always repeated at least five or six times; and the determination was made by comparing the results with those of as many alternate similar experiments on water.

In reality this does not render the determinations laborious, except in appearance; for in order to compare experiments made on a solution with those made on water, several days intervening between the two series, it would be necessary to compare almost daily the thermometer of the calorimeter and the heated thermometer with a standard one; now this comparison, to attain the necessary degree of precision, would take at least as much time as the experiments relative to the determination of the specific heats.

In short, it ought to be remarked that this mode of operating not only has the advantage of rendering all correction needless, and of avoiding the errors that might result from a displacement of the zero in the thermometers used, but also renders insensible those which might be caused by an imperfection in the graduation of the thermometers—an imperfection which it is impossible completely to avoid in experiments where an error of $\frac{1}{100}$ of a degree has a very sensible influence on the results. In comparative experiments on water and another liquid, if the heating is the same in both cases, and if the initial and final temperatures are, within a few hundredths, the same in the one as in the other, the determination is affected by the errors of graduation of the thermometer to the exceedingly slight amount of only a few hundredths of a degree.

As the promised speedy publication of the memoir of M. Thomsen on the same subject diminishes greatly the importance of my own researches, I do not purpose to give in detail the numbers of all my experiments, but shall merely indicate the mean results.

I will nevertheless, in order to give the means of judging the course I have pursued and the agreement presented by the different observations, state in detail the experiments which refer to two series of determinations. I take as examples those on the specific heat of the solutions of hydrochloric acid containing, to one molecule of acid ($\text{HCl}=36.5$), 50 and 100 molecules of water (900 and 1800) respectively.

The study of these two solutions having taken place on two consecutive days, under absolutely similar atmospheric conditions, I have united in a single series all the experiments made on water.

The calorimeter used was the glass vessel equivalent to 14·9 grammes of water. The quantity of water was 170 grms. for each experiment. The heated thermometer was immersed at a temperature $T=65^{\circ}\cdot 10$. I denote by t and t' the initial and final temperatures of the calorimeter, and by r the ratio $\frac{T-t'}{t'-t}$.

Water, 170 grammes.

t .	t' .	$t'-t$.	$T-t'$.	r .
19·78	23·26	3·48	41·84	12·023
19·86	23·35	3·49	41·75	11·963
20·06	23·53	3·47	41·57	11·980
20·12	23·57	3·45	41·53	12·037
19·80	23·28	3·48	41·82	12·017
19·69	23·18	3·49	41·92	12·011
20·04	23·505	3·465	41·595	12·004
19·91	23·385	3·475	41·715	12·004

The mean of all the above values of r is $=12\cdot 005$. The calorimeter and the contained water being equivalent to 183 grms. of water, 15·402 grms. are deducted as the equivalent of the heated thermometer.

H Cl + 50 Aq, 182 grms.

t .	t' .	$t'-t$.	$T-t'$.	r .
19·92	23·40	3·48	41·70	11·983
20·01	23·49	3·48	41·61	11·957
20·10	23·57	3·47	41·53	11·968
20·08	23·54	3·46	41·56	12·011
19·92	23·39	3·47	41·71	12·020
19·92	23·38	3·46	41·72	12·057

Mean . . . 12·000

We have, therefore, for the specific heat of this solution :—

$$C = \frac{15\cdot 402 \times 12 - 14\cdot 9}{182} = 0\cdot 9336.$$

H Cl + 100 Aq, 176 grms.

t .	t' .	$t'-t$.	$T-t'$.	r .
19·81	23·29	3·48	41·81	12·014
19·73	23·22	3·49	41·88	12·000
19·92	23·40	3·48	41·70	11·983
19·98	23·45	3·47	41·65	12·003
20·02	23·495	3·475	41·605	11·973

Mean . . . 11·995

$$C = \frac{15\cdot 402 \times 11\cdot 995 - 14\cdot 9}{176} = 0\cdot 9650.$$

I do not think the error of these determinations can exceed 0·002, including that which may depend on the preparation of the solutions, to which, however, I devoted the greatest care.

Having given my reasons for thinking my method very accurate as well as very simple, I am bound to mention a grave inconvenience attending it, which I do not regard as insurmountable, but which I have not yet succeeded in removing, and have consequently been for some time hindered in the application of the process.

The construction of thermometers with reservoir in the form of a very close helix is attended with great difficulties; and the clever maker who supplied me with them appears to have been obliged to take for the purpose tubes of rather thick glass. It results from this thickness, and the torsion they have undergone, that they are extremely apt to break when subjected to the abrupt changes of temperature produced by the sudden immersion, although these changes have never exceeded 45° . None of them has been capable of withstanding a very long series of experiments. Yet I think it would be possible to make them of thinner glass, so that they could support these abrupt transitions.

Wishing to determine the specific heats of some solutions in sulphide of carbon, I was obliged to abandon the preceding method, which was unsuitable for so volatile a liquid. In this case I operated in the following manner:—

The solution is contained in a glass balloon of 40 cubic centims. capacity. A very sensitive thermometer indicates the temperature. The neck of the balloon is sufficiently narrow to be nearly filled by the stem of the thermometer; a thin tube of caoutchouc, squeezed between the two, closes it exactly.

The liquid having been heated to a temperature a little above that at which the experiment is to commence, is cooled by continual agitation; and the moment the thermometer marks the degree determined, the whole is immersed in the calorimeter, which is arranged as in the preceding experiments and contains from 140 to 150 grms. of water. Of this a second thermometer indicates the initial temperature (which should be quite stationary under the influence of exterior causes), and its rise in consequence of the heat communicated by the heated solution. Care is also taken to agitate the balloon in the calorimeter continually till the maximum is reached.

Further, all correction is dispensed with, as before, by comparison with other experiments, under the same conditions, made with a liquid of known specific heat.

This process does not yield such accurate results as the preceding, on account of the greater duration of the experiments; for the maximum, instead of being attained in 30 seconds, is

only reached at the end of three or four minutes, so that the slightest difference in the action of exterior causes, or in the agitation of the balloon, may exercise a perceptible influence on the result. Very concordant results are nevertheless obtained when the external temperature remains nearly constant, provided great care is taken to keep the temperature of the calorimeter, for more than five minutes, absolutely invariable.

By reason of the small specific heat of sulphide of carbon, I should have been obliged, if I had wished to make use of water in the experiments for comparison, to put only 10 grammes of it into the balloon, which would not have been sufficient to completely bathe the thermometer; and it is to be feared that, under these conditions, the experiments would have been hardly comparable. I therefore preferred to take for comparison sulphide of carbon itself, a liquid the specific heat of which is well known. M. Regnault has determined very carefully, and has given the following formula for the calculation of the quantity of heat necessary to raise its temperature from 0 to t° :—

$$Q = 0.2352314t + 0.00008143131t^2.$$

By means of this formula it is calculated that the mean specific heat, between 17° and 44° , the limits of my experiments, is 0.2374. The experiments of M. Hirn* indicate a rather higher number; for he finds 0.23878 for the specific heat at 30° . I have adopted the mean number 0.238.

Before commencing the determinations which relate to these solutions, I made a great number of preliminary determinations on water, in order to know the conditions necessary to be fulfilled to make them comparable.

I ascertained that as long as the quantity of water in the balloon remains the same, we may vary its temperature at the moment of immersion, even to a considerable degree, without producing any sensible change in the value of the ratio $r = \frac{T - t'}{t' - t}$; so that all the experiments made in these conditions lead to the same number for the equivalent in water of the balloon and the thermometer contained in it.

But if we change the weight of water in the balloon, we find a different number to express the equivalent in question, and the new series of experiments is not comparable with the former.

It is easy to account for these results, if we remark that there are two principal causes of loss of heat—the exterior radiation of the calorimeter, and the fact that, at the moment the maximum is indicated, the liquid in the balloon has still a temperature higher than that attributed to it in the calculation when it is supposed to be brought to the temperature of the calorimeter.

* *Ann. de Chim. et de Phys.* 4th Series, vol. x. p. 84.

Now the time occupied by the experiments remains the same whatever the elevation of temperature obtained, since the maximum is attained, towards the end, so much the more quickly as the excess of temperature, being greater, determines a stronger radiation. Hence the first of the causes of loss above mentioned is nearly proportional to the elevation of temperature attained, so that the ratio of the variations of temperature of the balloon and of the calorimeter is always reduced by it in the same proportion; consequently the ratio remains constant for all the experiments of one and the same series.

As to the loss of heat produced by the second cause, it is proportional both to the amount by which the interior temperature exceeds the exterior (and this will be proportional to the height of the temperature of the calorimeter), and to the weight of water contained in the balloon.

It follows that only those experiments can be compared in which the liquid contained in the balloon represents the same value in water, but it is not necessary that the initial temperature shall be always the same.

Consequently I united in one series all the experiments made at different times on pure sulphide of carbon, to deduce from them the value of the balloon and the thermometer, only taking care to use always nearly the same weight of that liquid (about 43 grms.), and afterwards to take of the various solutions weights equivalent to that quantity as regards their specific heat.

In stating the results of my observations, I will commence with the aqueous solutions.

For the formulæ of compound bodies I have adopted the atomic notation, hydrogen being taken as unity; they are therefore identical with those of M. Thomsen.

In the Tables of results I designate by

n the number of molecules of water to one molecule of the substance dissolved;

c the specific heat of the solution per unit of weight;

p the molecular weight of the solution;

$C = pc$ the molecular heat, both that given by experiment, and that resulting from an empiric formula indicated further on for each case.

A final column gives the value of $C - 18n$; that is, the difference between the molecular heat of the solution and that of the water contained in it. To obtain these differences I have used the values of C calculated by means of the interpolation-formula, in order that the law of their progression might not be masked by the uncertainty of the values of C given by experiment in the case of very dilute solutions.

Although I give in the Tables the value of c to four decimals,

to represent with fidelity the means of my experiments, I admit that the fourth may very well be suppressed; but I think that the third should be, within one or two units, accurate.

Sulphuric Acid.

The acid used in my experiments had been purified by distillation and brought exactly to the state of a monohydrate by repeated congelation, until it no longer presented any sign of melting at the temperature of 10° .

For the monohydrated acid, and the mixtures of it with 5, 10, 15, and 25 equivalents of water, it was necessary to use a glass vessel instead of the metallic one which served for the other experiments. The results are comprised in the following Table:—

n.	c*.	p.	C		C - 18n.
			Observed.	Calculated.	
0	0.3315	98	32.5		
5	0.5764	188	108.4	108.4	+18.4
10	0.7212	278	200.5	200.5	20.5
15	0.7919	368	291.4	290.2	20.2
25	0.8537	548	468	468	18
50	0.9155	998	914	914	14
100	0.9545	1898	1812	1812	12
200	0.9747	3698	3604	3610	10
400	0.9878	7298	7209	7209	9

The empiric formula by means of which the above results were obtained, and the specific heats of any other solution may be calculated, is:—

$$C = 18n + 8.58 + \frac{334.8}{n} - \frac{2882}{n^2} + \frac{7262}{n^3}.$$

But it must be remarked that for solutions containing less than five molecules of water it would give quite inaccurate results.

Sulphuric acid is one of the bodies the specific heat of which has been already made known by M. Thomsen†. His experiments are not directly comparable with mine, because he refers his solutions to anhydrous sulphuric acid, to which he supposes added 5, 10, 20, &c. equivalents of water. But it is easy to compare his results with those deduced from the preceding formula for the molecular heats. We thus find, n having the same signification as in the preceding Table:—

* These values refer to a temperature of from 16° to 20° .

† *Archives des Sciences Phys. et Nat.* vol. xxxix. p. 153.

<i>n.</i>	Calculated.	Thomsen.
4	92·7
9	182·2	182·0
19	361·2	360·8
49	896·2	899·6
99	1794	1795
199	3592	3591

The agreement is as satisfactory as could be expected.

I feared that the method employed in these determinations, very suitable for saline solutions or for mixtures of acid with water in excess, would involve much uncertainty in the case of sulphuric acid monohydrated or mixed with a very small proportion of water.

Indeed, such a liquid tending, instead of evaporating in contact with the air, to absorb humidity from it and become heated, the experiments are no longer made in identical conditions with those of the experiments for comparison on water; so that, to obtain a temperature stationary at the beginning of an experiment, the temperature of the acid-bath must be about 4° above the atmospheric. Besides, the great difference in specific heat of the liquids obliges us to use very different volumes of them.

This fear induced me to repeat the determinations for the monohydrated acid, alone and with from one to five equivalents of water, by the second method—the one I used for the solutions in sulphide of carbon.

But then another difficulty is encountered. Either on account of the viscosity of the concentrated acid, or because the greater volume we are obliged to put in the balloon in order to have a calorific mass equal to that of the water prevents the different parts from being so well mixed by the agitation, the transmission of the heat to the water of the calorimeter takes place more slowly. Now, if the experiment lasts longer, the losses by radiation are no longer identical. This inconvenience can be remedied, after some trials, by so regulating the velocity of the agitation of the balloon in the calorimeter that each of the experiments shall occupy the same time.

This second series of experiments gave the following results, expressing the specific heat between 20° and 56°:—

<i>n.</i>	<i>c.</i>	<i>p.</i>	<i>C.</i>	<i>C</i> − 18 <i>n.</i>
0	0·3363	98	33	
1	0·4411	116	51·2	33·2
3	0·5056	152	76·8	22·8
5	0·5833	188	109·7	19·7

Let us now compare these results with those of the preceding experiments or obtained by other observers.

$\text{H}^2\text{O SO}^3$. I found:—

$c=0.3315$ between 17° and 22° ;

$c=0.3363$ „ 20 „ 56.

These numbers confirm one another, their difference being just that which should result from the difference of the temperatures.

Previous determinations had given:—

0.3095, Person*.

0.343 between 21° and 46° , H. Kopp†.

0.3413 „ 13 „ 77, Pfaundler‡, old determinations.

0.355 „ 22 „ 80, Pfaundler§, recent experiments.

It may also, by analogy, be concluded, from the whole of the observations of the last-mentioned savant with respect to the highest temperatures, that the result would have been about 0.352 between 22° and 60° .

We see that my results are a little lower than those obtained by MM. H. Kopp and Pfaundler. Supposing that they used in their experiments sulphuric acid merely concentrated by prolonged boiling, this alone would go far to explain the difference. In fact, such an acid contains $\frac{1}{2}$ of an equivalent of water too much. We should therefore have, starting from the result I obtained for the monohydrated acid (namely, $c=0.3363$ and $C=33$):—

99.5 for the molecular weight of the acid concentrated by boiling,

34.5 for its molecular heat, and

0.3467 for its specific heat.

$\text{H}^2\text{O}, \text{SO}^3 + \text{Aq.}$ I find $c=0.4411$ between 20° and 56° .

This result agrees exactly with the last observations of M. Pfaundler; for he obtained

0.447 between 22° and 80° ,

0.444 „ 22 „ 70.

$\text{H}^2\text{O}, \text{SO}^3 + 5 \text{Aq.}$ I obtained

0.5764 between 15° and 19° ,

0.5833 „ 20 „ 56.

The difference between these results appears to correspond well with that between the temperatures.

The results, therefore, of these determinations seem to me to confirm those of the first series.

It will moreover be remarked that these experiments confirm

* *Ann. de Chim. et de Phys.* S. 3. vol. xxxiii. p. 437.

† *Pogg. Ann.* vol. lxxv. p. 98.

‡ *Journ. für prakt. Chem.* vol. ci. p. 507.

§ *Berichte der Deutsch. chem. Ges. zu Berlin*, 1870, p. 798.

completely the curious fact established by M. Pfaundler, that the first equivalent of water added to monohydrated sulphuric acid increases the specific heat by a quantity precisely equal to that of the water added, while for every further dilution there is a very considerable loss of specific heat. This is probably connected with the existence of the bihydrate as a perfectly definite compound.

Sulphate of Soda, $\text{Na}^2\text{O}, \text{SO}^3 + n \text{ Aq.}$

<i>n.</i>	<i>c.</i>	<i>p.</i>	C		C-18 <i>n.</i>
			Observed.	Calculated.	
50	0·8890	1042	926·3	926·3	+26·3
100	0·9345	1942	1815	1815	+15
200	0·9625	3742	3602	3602	+ 2
400	0·9805	7342	7199	7193	- 7

A solution with 40 equivalents of water crystallizing at the ordinary temperature. I have not been able to operate on solutions more concentrated than that containing 50 equivalents.

The first three determinations lead to the formula

$$C = 18n - 16\cdot34 + \frac{4094}{n} - \frac{98000}{n^2}.$$

We see that for solutions containing more than 200 molecules of water the specific heat is less than that of water alone, and that the specific heat is always diminished by the addition of water to a solution of sulphate of soda.

Observations on the specific heats of these solutions have already been published by M. Schüller*. His experiments were not made on solutions of the same standard as those which I examined; but he has deduced from his results an empiric formula by which the specific heat may be calculated for all proportions of salt dissolved. This calculation, applied to the solutions which I have studied, would give the following results, which I have placed opposite to those obtained by me:—

	Observed.	By Schüller's formula.
$\text{Na}^2\text{O}, \text{SO}^3 + 50 \text{ Aq}$	0·8890	0·8916
„ + 100 „	0·9345	0·9392
„ + 200 „	0·9625	0·9678
„ + 400 „	0·9805	0·9835

The differences, especially for the solutions with 100 and 200 equivalents of water, appear to me to exceed the errors possible in my experiments.

* Pogg. Ann. vol. cxxxvi. p. 70.

Bisulphate of Soda : $\left. \begin{matrix} \text{Na} \\ \text{H} \end{matrix} \right\} \text{O, SO}^3 + n \text{ Aq.}$

<i>n.</i>	<i>c.</i>	<i>p.</i>	<i>C</i>		<i>C</i> - 18 <i>n.</i>
			Observed.	Calculated.	
25	0·8683	570	495	495	+ 45
50	0·9146	1020	933	933	+ 33
100	0·9497	1920	1823	1823	+ 23
200	0·9719	3720	3615	3618	+ 18

The empiric formula calculated from the first three determinations is

$$C = 18n + 11\cdot65 + \frac{1292}{n} - \frac{11500}{n^2}.$$

It is interesting to compare the molecular heats of solutions, at the same strength, of sulphate of soda, bisulphate, and sulphuric acid.

<i>n.</i>	$\text{H}^2\text{O, SO}^3.$	$\left. \begin{matrix} \text{H} \\ \text{Na} \end{matrix} \right\} \text{O, SO}^3.$	$\text{Na}^2\text{O, SO}^2.$
25	468	495	
50	914	933	926
100	1812	1823	1816
200	3610	3618	3602
400	7210	7194

The molecular heat of solutions of bisulphate is always more than the half of the sum of those of sulphuric acid and the neutral sulphate. The specific heat is therefore always augmented by the mixture of solutions of sulphuric acid and sulphate of soda. This fact is connected with the lowering of temperature produced by that mixture.

Hydrochloric Acid : $\text{HCl} + \text{Aq.}$

<i>n.</i>	<i>c.</i>	<i>p.</i>	<i>C</i>		<i>C</i> - 18 <i>n.</i>
			Observed.	Calculated.	
6·25	0·6687	149	99·6	99·6	- 12·9
12·5	0·7881	261·5	206·1	206·1	- 18·9
25	0·8787	486·5	427·5	426·8	- 23·2
50	0·9336	936·5	874·3	874·3	- 25·7
100	0·9650	1836·5	1772	1773	- 27
200	0·9835	3636·5	3576	3572	- 28

The molecular heat may be calculated by the formula

$$C = 18n - 28\cdot39 + \frac{140}{n} - \frac{268}{n^2}.$$

Hence the specific heat of solutions of chlorhydric acid is

always less than that of the water by itself which they contain. This is true even of the most concentrated solution I have examined, which contained 24.5 for 100 of acid. There is thus always a diminution of specific heat by the mixture of the water with these solutions.

Hydrochloric acid is one of the bodies for which the results obtained by M. Thomsen have been already published. Here is the comparison of the molecular heats found by that savant with those indicated by the formula deduced from my experiments :—

<i>n.</i>	Thomsen.	Calculated.
10	162	162.9
20	338.5	338
50	872	874
100	1769	1773
200	3557	3572

The differences little exceed the errors possible on the one side and the other, except for the solution with 200 molecules of water.

The result obtained by M. Thomsen for this solution appears to me very low: it would suppose a singular anomaly in the law of the progression of the differences $C - 18n$.

Chloride of Sodium : $\text{NaCl} + n \text{ Aq.}$
C

<i>n.</i>	<i>c.</i>	<i>p.</i>	C		$C - 18n$.
			Observed.	Calculated.	
12.5	0.8100	283.5	229.6	229.6	+ 4.6
25	0.8760	508.5	445.5	445.5	— 4.5
50	0.9280	958.5	889.5	888.3	— 11.7
100	0.9596	1858.5	1783	1784	— 16
200	0.9782	3658.5	3578	3582	— 18

The molecular heat is expressed by the formula

$$C = 18n - 20.45 + \frac{481}{n} - \frac{2100}{n^2}.$$

Inspection of the last column shows that the dilution of a solution of chloride of sodium always occasions a diminution of the molecular specific heat. A solution containing about 18 molecules of water to 1 of the salt has the same specific heat as the water which it contains; but the specific heat of more dilute solutions is less than that of the water contained in them.

Solutions of chloride of sodium have also been studied by M. Schüller. From his experiments their specific heat may be expressed by the formula

$$C = 0.9624 \frac{100 + 0.214p}{100 + p},$$

p denoting the weight of the salt dissolved in 100 parts of water, and 0.214 the specific heat of chloride of sodium in the solid state.

This formula is evidently inaccurate for very dilute solutions, since the conclusion from it would be that the specific heat of those solutions could not exceed 0.9624, however minute might be the proportion of the salt. From this formula, the following would be the specific heats of the solutions examined by me:—

n .	p .	Calculated.	Observed.
12.5	26	0.8063	0.8100
25	13	0.8754	0.8761
50	6.5	0.9162	0.9282
100	3.25	0.9386	0.9592
200	1.62	0.9503	0.9785

With respect to solutions containing more than 25 molecules of water the disagreement is absolute.

Sugar: $C^{12}H^{22}O^{11} + Aq^*$.

n .	c .	p .	C .	$C - 18n$.
25	0.7558	792	598.6	148.6
50	0.8425	1242	1046	146
100	0.9091	2142	1947	147
200	0.9500	3942	3745	145
400	0.9742	7542	7347	147

Within the limits of errors of experiment, the numbers in the last column may be regarded as equal. The conclusion hence is, that the specific heat of a mixture of water and a solution of sugar is always sensibly equal to the sum of those of the two liquids mixed.

It may therefore be admitted that the specific heat of any solution of sugar is the sum of those of the water and the sugar, that the mean number 147 expresses the molecular heat of sugar in the liquid state, and that its specific heat per unit of weight is consequently 0.430.

According to M. H. Kopp, the specific heat of sugar in the solid state is 0.301, its molecular heat 103. The ratio of these numbers to those belonging to the liquid state offers nothing abnormal.

Solutions in Sulphide of Carbon.

As mentioned at the commencement of this memoir, all the experiments on these solutions were compared with those made

* Theoretical considerations, to which I shall return at the close of this memoir, made me desirous of studying solutions not belonging to the group of salts, acids, and bases. In consequence of the difficulty of finding such bodies presenting in addition the conditions of great solubility, perfectly definite composition, and not combining with water, the aqueous solution of sugar is the only one I am aware of suitable for these experiments. I used very pure sugar candy.

on pure sulphide of carbon, taking for the specific heat of this liquid the number 0.238. Five series of observations, made during these researches, served to determine the equivalent in water of the balloon containing the liquid, with the interior thermometer, which showed the temperature stationary at $43^{\circ}59$ at the moment the balloon was immersed in the calorimeter.

The calorimeter, in every case, contained 140 grms. of water, to which must be added 13 grms. as the equivalent of the vessel and the thermometer. The balloon contained about 44 grms. of sulphide of carbon, or of each solution a weight determined by a preliminary experiment to obtain always the same heating of the calorimeter.

Sulphur: $S + n CS^2$.

<i>n</i> .	<i>c</i> .	<i>p</i> .	<i>C</i> .	$C - 18.1n$.
1	0.229	108	24.7	6.6
2	0.232	184	42.8	6.6
4	0.232	336	77.9	5.5
10	0.235	792	186.0	5.0

18.1 represents the molecular heat of sulphide of carbon; the last column expresses therefore the amount by which the presence of an atom of sulphur increases the specific heat of the solution.

According to M. Person, the specific heat of melted sulphur, between 120° and 150° , is 0.234; its atomic heat is therefore 7.5; but the number would certainly be less, if it could be determined at a temperature of from 18° to 43° .

Thus the atomic heat of sulphur dissolved in sulphide of carbon is only a little less than that of melted sulphur, and is diminished in only a slight proportion by a considerable dilution: in fact the last solution contains only 4 per cent. of sulphur.

Phosphorus: $P + n CS^2$.

<i>n</i> .	<i>c</i> .	<i>p</i> .	<i>C</i> .	$C - 18.1n$.
$\frac{1}{4}$	0.219	50	10.9	6.4
$\frac{1}{2}$	0.222	69	15.3	6.3
1	0.225	107	24.2	6.1
2	0.229	183	41.9	5.7
4	0.2295	335	76.9	4.5

According to M. Person, the specific heat of melted phosphorus would be 0.2045, consequently its atomic heat 6.3.

Phosphorus enters, then, with all its specific heat into concentrated solutions in sulphide of carbon. This heat diminishes gradually, but very slightly, in solutions more and more dilute.

Bromine.—I first determined, as a means of control, the specific heat of liquid bromine compared with sulphide of carbon. For this purpose I had to operate on 90.5 grms. of bromine.

For its specific heat between 18° and $43^{\circ}6$ I found 0.1125.

This number agrees exactly with the determinations of M. Regnault, who found 0.11294 between 13° and 58°. The atomic heat is therefore equal to 9.

For the specific heat of the mixture $\text{Br} + \text{CS}^2$ (51 per cent. of bromine) I found 0.174, and consequently 27.1 for the molecular heat. This is exactly the sum of the heats of the two liquids in the mixture.

Iodine: $\text{I} + n \text{CS}^2$.

<i>n.</i>	<i>c.</i>	<i>p.</i>	<i>C.</i>	$\text{C} - 18.1n.$
10	0.219	887	194	13
20	0.228	1647	376	14

According to MM. Favre and Silbermann, the specific heat of fused iodine is 0.1082; therefore its atomic heat is 13.7.

Thus iodine is dissolved in sulphide of carbon without its specific heat being sensibly diminished.

A solution with 10 molecules of sulphur, containing 14.3 per cent. of iodine, is very near saturation. I could not operate on more concentrated liquids.

We see that all these solutions present a character very different from that observed in the aqueous solutions of acids and salts. The diminution of specific heat resulting from solution, or from dilution when this exists (as in the case of sulphur and phosphorus), is always within narrow limits. Above all, the molecular heat of these solutions never becomes inferior to that of the solvent alone.

General Observations.

In the preliminary summary of his great work on the specific heats of aqueous solutions, M. Thomsen announces that he can deduce from his observations the general conclusion that a diminution of specific heat always results from the mixture of water with a solution.

The generality which he attributes to this law is perhaps dependent on the fact that all his researches were made upon combinations belonging to one class (hydrated salts, acids, and bases). It is known, in fact, that a mixture of water and alcohol has a specific heat superior to that of its elements. It results from the facts reported in this memoir that solutions of sugar have a specific heat equal to that of their elements. It may even be remarked that among the few bodies for which M. Thomsen has published the results of his experiments there is one which behaves like sugar, viz. ammonia; certainly the differences do not reach the limits of error admitted by him as possible.

But, with this reserve, it is nevertheless true that this diminution of specific heat is very general, and that its proportion is often very considerable. Thus we have seen that a solution of

hydrochloric acid containing 24·5 per cent. of acid, or one molecule for 6·25 of water, has a specific heat about 11 per cent. less than that of the water alone which it contains.

This is a very remarkable fact. Indeed, some years ago* M. H. Kopp showed, after a general recapitulation of the researches made by himself and his predecessors on the specific heats of combinations, that the molecular specific heat of any definite compound, organic or inorganic, is nearly equal to the sum of the specific heats of its elements,—the differences being either of the same order as the errors of the determinations or as the differences which may result from a simple change in the physical state of one and the same body. We know, besides, from the experiments of M. Regnault on alloys (that is, on the combinations which, by their indefinite composition, seem to come the nearest to solutions), that they are subject to the same approximate law.

It seems, then, that we are justified in regarding this as a general principle. How is it that it does not apply to solutions? Two hypotheses may be made on this subject:—

This anomaly may be due to a purely physical cause. The laws resulting from the observations of MM. Regnault and Kopp refer to the solid state of compound bodies; perhaps they are true for that state only.

But if this were the fact, all solutions ought to behave in the same manner and present an analogous diminution of specific heat in proportion as the dilution is increased. Now we have just seen that it is not so. There is sometimes an increase in the specific heat; and in other cases it remains equal to that of the liquid mixed. The chemical nature of the bodies evidently exercises a great influence on these phenomena; they must, then, have a chemical cause.

We are thus led to the second hypothesis. The specific heat of a solution ought always to be equal to the sum of the specific heats of the bodies mixed, except the small variations resulting from modification of the physical properties (cohesion, dilatability, &c.). But most solutions have a very unstable chemical constitution, and one that varies both with the degree of dilution and with the temperature. Every change of temperature involves, then, a change in the chemical constitution, a chemical work, which is itself the source of an absorption or a disengagement of heat. This, of necessity, augments or diminishes by so much that which must be transmitted to the solution to change its temperature, and consequently its apparent specific heat. Therefore the difference between the specific heat of a solution and the sum of those of its elements will be the proof, and ap-

* *Annalen der Chemie und Pharmacie*, Suppl. vol. iii. p. 1.

proximately the measure, of the work produced by the chemical reaction determined by the change in the temperature of the liquid.

We ought hence to conclude that, for example, when a mixture of water and alcohol is heated, there is produced between these bodies a reaction by which heat is absorbed, while solutions of saline compounds undergo, when their temperature is raised, a chemical transformation which disengages heat; finally, certain solutions (as the aqueous ones of ammonia and sugar, and the solutions of simple bodies in sulphide of carbon) should present no change in their chemical constitution at different temperatures.

It would be venturing too far into the domain of hypothesis to attempt to state precisely the nature of the chemical reactions that may be determined by change of temperature in a solution. It may be that they affect only the state of combination, more or less intimate, of the solvent and the body dissolved; but it is possible that they may be of a nature more profound, and in certain cases affect even the composition of the body dissolved—for example, that a chloride may be transformed into a hydrochlorate. It is certain, however, that if two classes be made, the first including those bodies the dissolving of which induces a considerable change in their specific heats, the second those which are not so affected, it will be remarked that the latter are of such a nature that it is impossible to suppose any alteration produced in their chemical constitution, and that nothing seems to indicate that they can form definite combinations with the solvent*; while the same cannot be said of the saline compounds, acid or basic, all of which belong to the first class.

Besides, I do not pretend to be the first to advance this hypothesis. I cannot say where it is first to be found; but the same idea is expressed by M. Pfaundler in a note on the specific heats of the first three hydrates of sulphuric acid†. Having ascertained that the addition of one molecule of water to the monohydrated acid augments the specific heat by a quantity equal to that of the water added, while the addition of a second molecule only increases it by a little more than half that quantity, he thence concludes that probably the action of the heat determines a chemical modification in the solution

* Ammonia (which, from M. Thomsen's experiments, belongs to this second class) seems, at first view, to form an exception, its chemical character giving an air of probability to the notion that it would combine with water; nevertheless the properties of the solution, and its total decomposition by spontaneous evaporation, have always caused the rejection of the idea that it could be regarded as a combination.

† *Journ. für prakt. Chem.* vol. ci. p. 507.

containing three molecules of water, which changes its apparent specific heat.

Pursuing the same hypothesis, he has more recently* proposed the study of the specific heats of solutions and their elements as a means of determining the degree of dissociation produced in them by heat. I cannot, however, share his opinion of the results to be expected from this study; for his calculations rest upon a principle the accuracy of which is not demonstrated—namely, that the heat disengaged by the total combination of the two bodies would be the same at all temperatures.

It must be acknowledged that this hypothesis of a change in the chemical constitution of solutions according to the temperature removes a great difficulty. It very easily accounts for what takes place in a mixture of water and alcohol. The combination of these two bodies disengages heat; it seems very natural to admit that the rise of temperature of the mixture determines a dissociation, which must necessarily absorb heat and consequently increase the apparent specific heat. On the contrary, it is difficult to conceive that the rise of temperature of a mixture of water and sulphuric acid determines a chemical action that disengages heat—that is to say, a more intimate combination.

Yet I do not think that this difficulty ought to cause the rejection before-hand of this hypothesis as baseless; and it appears to me useful to enunciate or recall these theoretic ideas at a time when several able investigators are directing their researches to these questions.

I will add another consideration which seems to me to demonstrate well the existence of this internal chemical work, determined by the change of temperature, in certain solutions. Let us compare water and a saline solution, that of chloride of sodium for example. This is less compressible than water; therefore more work must be consumed to produce an equal change of volume without alteration of temperature. But, on the other hand, its coefficient of dilatation is much higher than that of water, and its specific heat less. For these two reasons a much less quantity of heat is necessary to produce the same change of volume accompanied by a rise of temperature. These two facts, apparently contradictory, can only be explained by admitting that an internal chemical work, determined by the rise of temperature, has furnished a part of the heat necessitated by the dilatation.

In a subsequent article I will exhibit the results of my study of the densities and dilatations of the aqueous solutions the specific heats of which have been determined in this memoir.

* *Zeitsch. für Chemie*, year xiii. p. 66.

XXII. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from p. 78.]

June 22, 1870.—Joseph Prestwich, Esq., F.R.S., President,
in the Chair.

THE following communications were read:—

10. "On an altered Clay-bed and Sections in Tideswell Dale, Derbyshire." By the Rev. J. M. Mello, M.A., F.G.S.

The author describes the sequence of the rocks seen in a quarry in Tideswell Dale as follows:—Beneath a thin layer of surface-soil is a bed of Toadstone, containing concretionary balls, and much decomposed above; beneath this is Toadstone in large blocks of indefinite shape, very hard, dark-green, and apparently doleritic, nine or ten feet thick, passing downwards into a coarse and much decomposed bed, partly amygdaloid, partly vesicular, about 1 foot thick. Beneath the Toadstone rocks, and without any sharp line of demarcation, is a thick bed of indurated red clay, 3 yards in thickness, presenting a regularly prismatic columnar structure, resting on a thin bed of greenish-yellow clay containing fragments of limestone, which covers beds of good Derbyshire marbles containing corals. The author suggests that the columnar clay-bed may perhaps be a local development of that which forms partings in the limestone near Litton Tunnel.

11. "On the Physics of Arctic Ice as explanatory of the Glacial Remains in Scotland." By Dr. Robert Brown, M.A., F.R.G.S., &c.

In this paper the author entered into an extended inquiry how far the formation of the boulder-clays and other glacial remains in Scotland and the north of England can be accounted for on the theory of a great ice-covering having at one time overlain the country in much the same manner as it does now Greenland and other extreme Arctic countries. Taking the hypothesis of Agassiz as his groundwork, Dr. Brown entered into a minute description of the present glacier-system of Greenland, and the nature of Arctic ice-action, and into an inquiry how far glacial remains in Britain correspond with those at present in course of formation in Greenland and at the bottom of Baffin's Bay, Davis Straits, and the fjords and bays adjoining these seas. These inquiries were commenced in the year 1861, and have been continued at intervals ever since, up to the present summer, in various portions of the Arctic regions, the continent of Europe, in Great Britain, and in North America across to the Pacific. The result of these extended researches have led him to conclude:—1. That the subazotic boulder-clay corresponds with the *moraine profonde* which underlies glaciers, and in all likelihood is the immediate base on which the ice cap of Greenland rests. 2. That the fossiliferous, laminated, or brick-clays find their counterpart in the thick impalpable mud which the subglacial streams are pouring into the sea, filling up the fjords, even shoaling the sea far out, and, in some cases, absolutely turning the

glaciers from their course into other valleys. Allowing the very moderate computation that this impalpable mud accumulates at the rate of only six inches per annum, a deposit of fifty feet in a century must form.

If Scotland was at one time covered with an ice cap, or had glaciers of any extent (as cannot be doubted), then this deposit must have been equally forming, and as a geological formation must be accounted for. No difference could be detected between this glacial mud and the present brick-clays; and every fact went to show that it was to this that we must look for the formation of these laminated fossiliferous clays. The amount of earth deposited on the bottom by icebergs was very insignificant indeed, and could in no degree account for the *boulder-clay*, though it was shown that much of the *boulder-drift* in some places could be so accounted for. It was, however, demonstrated that there was a great distinction between the boulders which belonged to the *moraine profonde* and those which were carried off on icebergs as part of the ordinary lateral moraines.

The fjords, as already partially advocated in a paper in the Journal of the Royal Geographical Society (vol. xxxix.), he considered due to glacier action, the glaciers having taken possession of these fjords when they were mere valleys, when the coast was higher than now. He further showed that the American explorers are in error when they describe the coast of Greenland as rising to the north of 73°, and subsiding to the south of that parallel. There had been a former rise of the coast, and a fall was now in course of progress through the whole extent. Whether these had previously alternated with other rises and falls is not clearly evidenced by remains; but no doubt exists that a rise preceded the present subsidence. Numerous facts were adduced in support of this assertion. The remainder of Dr. Brown's paper was occupied in an attempt to apply the doctrines regarding the physical action of Arctic ice-action to account for the Scottish glacial remains, and to deduce therefrom evidence regarding the changes Scotland underwent during, and subsequent to, the glacial period.

ROYAL GEOLOGICAL SOCIETY OF IRELAND.

A paper was read by Edward Hull, Esq., F.R.S., Director of the Geological Survey of Ireland, "On the Geological Age of the Ballycastle Coal-field, and its relation to the Carboniferous Rocks of the West of Scotland." Read January 11, 1871.

The object of the paper was to prove that the coal-field of Ballycastle, co. Antrim, was referable to the type of the lower coal-field of Scotland, and consequently of the age of the Lower Carboniferous series—in other words, of the Mountain-limestone.

The Carboniferous series of Ballycastle, which had been described in 1829 by Sir R. Griffith, F.R.S.*, was shown to consist of three divisions, in descending order:—

* Report on the Coal-districts of Tyrone and Antrim, to the Royal Dublin Society, 1829.

1. *The Upper*, consisting of massive sandstones, shales, with beds of coal, black-band and clay-band, ironstone, &c. (*Lingula squamiformis*).
2. *The Middle*, consisting of a thin bed of limestone lying between shales, with carboniferous-limestone genera and species of shells, crinoids, and corals.
3. *The Lower*, consisting of massive reddish grit and conglomerate with thin beds of shale.

The author showed that the Carboniferous Limestone of Ireland undergoes in its extension northwards changes similar to those of the same formation in Britain when traced from Derbyshire into Northumberland and Scotland. The calcareous member thins away and is replaced by sedimentary strata of sandstone and shale, showing approximately terrestrial conditions productive of coal and ironstone. It was thus that in the case of the Glasgow coal-field the limestone of Derbyshire, several thousand feet in thickness, was represented by only thin bands of earthy limestone interstratified with a thick series of grits, shales, &c., with ironstone and coal. In a similar manner the Ballycastle coal-field, with only a few feet of limestone shown in the cliffs of the Bay, was the representative of the Carboniferous Limestone of the centre of Ireland, nearly 3000 feet in thickness.

Mr. Hull considered the lower division (No. 3) of the Ballycastle beds (as above described) to be undoubtedly the representative of the "calcareous sandstone series" of the Geological Survey, which lies at the base of the Carboniferous rocks of the West of Scotland, and that the middle and upper divisions (Nos. 2 and 1) correspond to the Carboniferous Limestone series, or lower coal-field of that country.

As regards the palæontological evidence, it was in favour of this view as far as it had been studied. Out of thirty-three species observed in the limestone band of Ballycastle Bay, 50 per cent. had been described in the Lower Carboniferous rocks of the West of Scotland; and one of the uppermost seams of coal lying above the limestone had yielded *Lingula squamiformis*, a form characteristic of the Limestone series in the north of England, Scotland*, and Ireland. Mr. W. H. Baily, F.G.S., concurred in the view of the age of these beds on palæontological grounds.

The author concluded by pointing out several features of similarity between the Ballycastle beds and the lower coal-series of the West of Scotland, such as the occurrence of several beds of "black-band" ironstone, the hydraulic and earthy character of the limestone of Ballycastle Bay, exactly resembling the "Arden" and "Cowglen" bands of Glasgow. Some uncertainty still remained whether there were any beds in the Ballycastle district as high in the geological series as the millstone-grit, or true coal-measures; but until more light could be brought to bear on this question by further exploration, and a complete investigation by the Government surveyors, the author meanwhile regarded the whole series as Lower Carboniferous.

* John Young, Trans. Geol. Soc. Glasgow, vol. ii.

XXIII. *Intelligence and Miscellaneous Articles.*

ON SOME HYDRO- AND THERMOELECTRIC FORCES, REDUCED TO SIEMENS'S UNIT OF RESISTANCE AND WEBER'S UNIT OF CURRENT. BY F. KOHLRAUSCH.

I HAVE made the present research in conjunction with M. A. Ammann. It refers to the electromotive forces of Grove's and of Daniell's elements, to the combination copper and zinc in dilute sulphuric acid, and the thermo-elements German silver and copper, copper and iron, and German silver and iron. Where it is not otherwise specially mentioned, the determinations were made by Poggen-dorff's compensation method. The electromotive forces are throughout expressed according to Ohm's law, $e=wi$, where the resistance w is expressed in Siemens's units, and the intensity i in the magnetic unit introduced by Weber. The electromotive forces thus measured I shall express by Siemens-Weber.

To measure the electromotive forces of the hydroelectric circuit, a tangent-compass of twenty-four turns was used, the mean diameter of which was 258.4 millims. The coils formed a circuit with a rectangular section 27 millims. in breadth and 9.4 millims. in height. The magnetic needle was a rectangular magnet 20 millims. in length with pointers affixed.

I will here give a formula for such a tangent-compass for the correction of the first degree, which can be frequently applied in galvanometrical measurements.

If r is the mean diameter of n circular coils which together form a circuit with a rectangular section of the breadth $2a$ and the height $2b$, if, finally, $2l$ is the distance of the poles of the needle from each other (meaning by pole the centre of the free unipolar magnetism), then the intensity of the current may be expressed in magnetic measure by the formula

$$i = \frac{rT}{2n\pi} \left(1 + \frac{1}{2} \frac{a^2}{r^2} - \frac{1}{3} \frac{b^2}{r^2} - \frac{3}{4} \frac{l^2}{r^2} \right) \tan \phi \left(1 + \frac{15}{4} \frac{l^2}{r^2} \sin^2 \phi \right).$$

T signifies here the horizontal intensity of the earth's magnetism, and ϕ the angle of deflection which the current i produces. It is presupposed that a , b , and l are small compared with r . The intensity of the earth's magnetism at the place of observation was compared with that in this magnetic observatory, and thence determined from the secular formula for Göttingen. T was then found = 1.902. The distance of the poles was assumed to be 19 millims. Hence for this tangent-compass

$$i = 1.631 \tan \phi (1 + 0.020 \sin^2 \phi).$$

A Siemens's scale was used as rheostat, it having been compared with our standards.

The electromotive forces were found by the method of compensation to be as follows:—

1. Grove's element (that is, platinum, concentrated nitric acid, sulphuric acid of 1.06 sp. gr., freshly amalgamated zinc)

$$= 19.98 \text{ Siemens-Weber.}$$

2. Daniell's element (that is, copper, concentrated solution of sulphate of copper, sulphuric acid, and zinc as above)

= 11.71 Siemens-Weber,

3. Copper, sulphuric acid, zinc as above,

= 10.82 Siemens-Weber.

The first electromotive force of the Grove's element was also determined by Ohm's method, that is, by measuring two intensities, different known resistances being inserted, in which the well-known enfeeblement of the electromotive force by the current manifests itself, although the intensities used only amounted to 1.7 and 0.9 Weber.

In Ohm's method the errors which, when a long needle is used, arise from the invalidity of the law of tangents are particularly prominent. Hence it may be remarked that they may be avoided if complementary angles to the two deflections of the needle are taken. Combined with the rule for accuracy, that one current must have about double the intensity of the other, it follows that it is most advantageous to use angles of 35° and 55° , by which at the same time the third condition of an accurate measurement is satisfied.

The *thermoelectric* forces were throughout determined by the method of compensation. By the aid of the electromotive force previously found for the Groves's element, and the great resistances which could be used on the Siemens's scale, the reduction-factor of a very delicate reflecting galvanometer was determined in Weber's unit. A second galvanoscope with an astatic needle, as well as the Siemens's scale, was used in the experiments to reduce the current in the thermoelement to zero. Siemens's standards of 1 to 4 units were inserted in the circuit of the galvanometer.

The *resistance of the galvanometer itself* was simply measured by determining the logarithmic decrement λ of the oscillating needle when the current was closed, and, further, the decrement λ' after a known resistance w' had been inserted in the circuit. Moreover the logarithmic decrement λ_0 , with an open circuit, was known; for then the desired resistance w of the multiplier is

$$w = w' \frac{\lambda' - \lambda_0}{\lambda - \lambda'}.$$

The metals investigated were in hard-drawn wires of about 1 millim. diameter. The copper was electrolytically deposited.

The results are found in the following formulæ; for if one junction has a temperature of about 16°C. , and the other one a temperature about t° higher, from the observations the electromotive force e is found in Siemens-Weber units.

German silver and copper.

$$e = 0.0001549t + 0.000000291t^2.$$

Copper and iron.

$$e = 0.0000969t + 0.0000000149t^2.$$

German silver and iron.

$$e = 0.0002476t + 0.000000196t^2.$$

The very close agreement of these three expressions found by *observation* (the latter is almost exactly the sum of the two former) gives at the same time a confirmation of the thermoelectromotive fundamental law. The differences are smaller than the possible errors of observation.

With what accuracy these expressions agree with the observations may be seen from the following comparison of some results obtained by observations with those calculated from the formula.

	<i>t.</i>	<i>E</i> observed.	<i>e</i> calculated.
German silver and copper.	{ 59·00	0·010156	0·010155
	{ 38·27	0·006348	0·006356
	{ 32·40	0·005335	0·005326
	{ 51·07	0·004561	0·004560
Copper and iron	{ 34·67	0·003193	3·003180
	{ 29·77	0·002739	0·002752
	{ 66·45	0·01725	0·01732
	{ 55·75	0·01453	0·01441
German silver and iron .	{ 43·07	0·01105	0·01103
	{ 32·80	0·00825	0·00833
	{ 21·80	0·00546	0·00549

Poggendorff's *Annalen*, No. 11, 1870.

ON THE DURATION OF FLASHES OF LIGHTNING.

BY O. N. ROOD. (FROM A LETTER TO DR. W. GIBBS.)

After the completion of my first set of experiments on the duration of the discharge of a Leyden jar, I became anxious to make some measurements of the duration of a flash of ordinary lightning, which may be considered equivalent to the discharge of an immense jar with an enormous striking-distance. The results of Feddersen have shown that the duration of the discharge is increased by an addition to the size of the jar, as well as by augmentation of the striking-distance; and as both these quantities are so large with a flash of lightning, it was reasonable to expect that the duration of its discharge would be prolonged in some corresponding ratio. During the violent thunder-storm of last August, which occurred in the evening, I happened to be at a house commanding an unobstructed view of the horizon; and this circumstance, taken in connexion with the frequency and proximity of the electrical discharges, induced me, although entirely unprovided with apparatus, to attempt a measurement of their duration. A circular disk, 5 inches in diameter, was hastily cut from white cardboard, while a steel shawl-pin served as an axis on which it was made to revolve by constantly striking its edge tangentially with the right hand, the pin being held in the left. The *maximum* velocity attainable in this way was always employed. The general indications at the time were that the rate thus obtained was considerably more uniform than

might have been expected; and subsequent quantitative experiments have confirmed this idea. The first experiments were made by observing black figures traced near the circumference of the disk, which was illuminated solely by the rapidly recurring flashes; and it often happened that the figures, with their details, were seen quite as clearly and sharply as though the disk had been stationary; on the other hand, sometimes the edges seemed blurred, as though the disk had moved through a few degrees during the act of discharge. The result being doubtful, the mode of experimenting was quickly changed; about fifteen narrow radial apertures were made near the circumference of the disk, and the flashes and illuminated clouds were observed through these openings, the disk being made to revolve as before. The distance of the eye from the apparatus was about 8 inches; and it was of course adjusted so as to obtain distinct vision of the disk. The result was that sometimes the openings were seen quite unchanged in appearance, but more frequently they were most distinctly elongated into well-defined streaks some degrees in length. They were observed often and without difficulty; but, as further confirmation, I may add that I requested Professor Joy, who was ignorant of the actual form of the aperture, to state his opinion of their apparent shape while the disk was in rotation. The reply was that they resembled Prince Rupert's drops, a not unfair description of the phenomena in question. Repeated estimates of their size were then made with paper and pencil. Some time afterward I measured the velocity which I could communicate to this disk in the manner above described, by attaching to it a small hollow axis through which the steel pin passed, the disk being then caused to wind up a thread stretched by a small weight. The rate of rotation thus attainable was found to be about twelve revolutions per second, which is a little more than I had anticipated. The average size of the streaks was 9° , corresponding to a duration of $\frac{1}{450}$ of a second. It hence results that the duration of the flashes of lightning on the occasion referred to was, in round numbers, about $\frac{1}{500}$ of a second, some of them, however, seeming to be confined to smaller limits.

I know of only a single circumstance which might militate against the correctness of the above conclusion; and it is but fair to give it such weight as it may carry. Becquerel has succeeded, with some difficulty, in observing a faint phosphorescence when an electric discharge is passed through rarefied air; and it is not absolutely impossible that the effects observed by me were due to a cause of this kind.

This point can hereafter readily be decided by observing with a revolving disk, not the distant clouds, but a sheet of white paper placed so as to receive the light from the electrical flashes.—Silliman's *American Journal*, January 1871.

THE
LONDON, EDINBURGH, AND DUBLIN
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

[FOURTH SERIES.]

MARCH 1871.

XXIV. *Calorimetric Researches.* By R. BUNSEN*.

[With a Plate.]

1. *The Ice-Calorimeter.*

THE calorimetric methods hitherto adopted are objectionable on account of the relatively large quantities both of the liquid needed for the calorimeter and of the substance examined, which have to be employed for the purpose of diminishing the error caused by heat unavoidably lost in the experiment. Satisfactory estimations of specific heat can scarcely be obtained by the more exact methods hitherto in use, unless the weight of substance employed is at least from 10 to 40 grms. Rare substances in a state of perfect purity can only be obtained in such quantities after almost insurmountable difficulties; and it is, only on this account, conceivable that we do not yet know the specific heats of all elements as yet obtained in the pure state, although such estimations are of fundamental importance in controlling the atomic weights.

The instrument about to be described will serve to remedy these objections. The principle on which it acts consists in measuring the volume of ice melted by the contraction which this ice undergoes on liquefaction.

The instrument (Plate V. fig. 1) consists of an inner glass vessel *a*, in the form of a common test-tube, which has been fused at the blowpipe into the cylindrical vessel *b*. From this vessel (*b*) proceeds the glass tube *c*, to which the iron collar (*d*) is fastened above. The inner vessel (*a*) is filled from *a* to μ , and

* Translated by Francis Jones, Esq., from Poggendorff's *Annalen*, No. 9, 1870.

the outer vessel (*b*) from β to λ , with boiled water; the remainder of the vessel *b* up to the level γ is filled with boiled mercury. In order to arrange the instrument for use, a cylinder of ice must be produced in the vessel *b* with which to surround the vessel *a* completely. For this purpose the whole apparatus is placed in a large vessel and surrounded with snow; the calibrated scale-tube, *s*, fitted accurately into the cork with fine sealing-wax, is then passed through the mercury in the collar (*d*), and made fast in the mouth of the tube (*c*), so that it becomes filled with mercury. That this may be accomplished without risk to the somewhat fragile apparatus, the instrument is secured in an iron support by means of a vice, which encloses firmly the lower part of the iron collar (*d*).

The amount of heat which a body gives up by being cooled to 0° C. is estimated by dropping the body into the water contained in the vessel *a*, which is then closed with a cork at δ to prevent change of air. If it behaves, on relative measurements of amount of heat, in the same way as in the estimations of specific heat, then this amount is obtained directly by observing the number of degrees on the scale to which the thread of mercury goes back. If the readings are to be converted into absolute quantities (for example, into grammes of melted ice or thermal units, viz. the amount of heat required to raise the temperature of 1 grm. of water at 0° C. to 1° C.), then the readings from the scale have only to be multiplied by a constant, which is derived from the following consideration:—

A thread of mercury measured in the scale-tube, and at the temperature *t*, and which assumes the temperature *T* after the calibrating-corrections have been made, weighs *g* grms. Further, let the specific weight of mercury at 0° C. be s_q , and its coefficient of expansion α , then the volume *v*, measured in cub. centims., of a corrected scale is

$$v = \frac{g(1 + \alpha t)}{s_q T}.$$

The instrument used by myself had the following:—

$$\begin{aligned} g &= 0.5326, \\ \alpha &= 0.0001815, \\ t &= 9^{\circ} \text{ C.}, \\ s_q &= 13.596, \\ T &= 507.4; \end{aligned}$$

and therefore

$$v = 0.00007733 \text{ cub. centim.} \quad . \quad . \quad . \quad (1)$$

Let s_e represent the specific gravity of ice at 0° C., s_w the

Fig. 2.

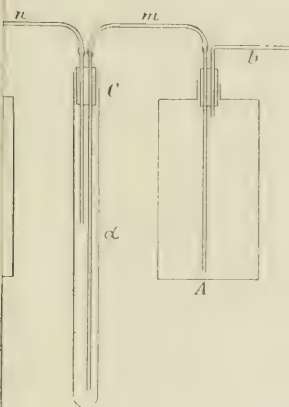


Fig. 5.

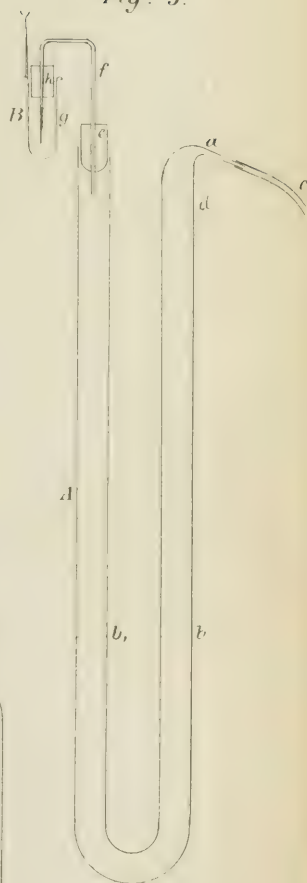
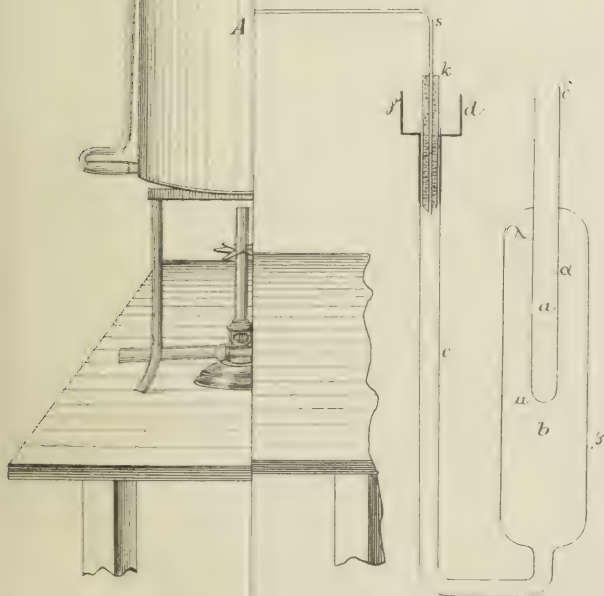


Fig. 1.



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The amount of heat which a body gives up by being cooled to 0° C. is estimated by dropping the body into the water contained in the vessel *a*, which is then closed with a cork at δ to prevent change of air. If it behaves, on relative measurements of amount of heat, in the same way as in the estimations of specific heat, then this amount is obtained directly by observing the number of degrees on the scale to which the thread of mercury goes back. If the readings are to be converted into absolute quantities (for example, into grammes of melted ice or thermal units, viz. the amount of heat required to raise the temperature of 1 grm. of water at 0° C. to 1° C.), then the readings from the scale have only to be multiplied by a constant, which is derived from the following consideration:—

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Fig. 4.

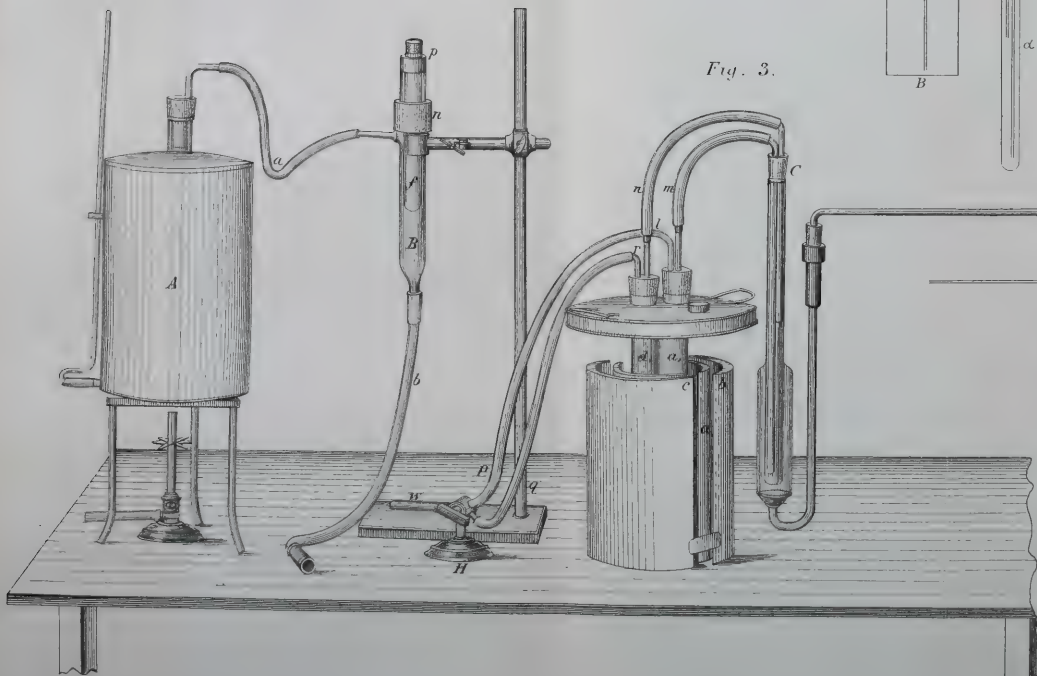


Fig. 3.

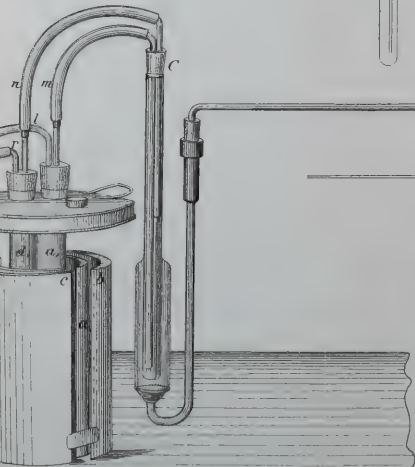


Fig. 2.

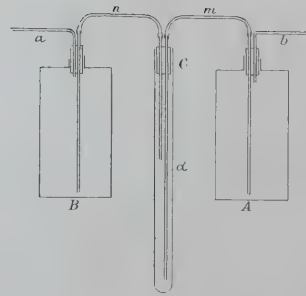


Fig. 5.

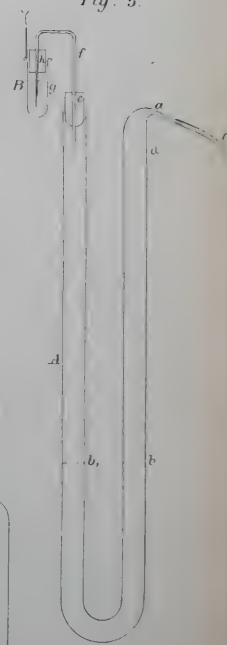
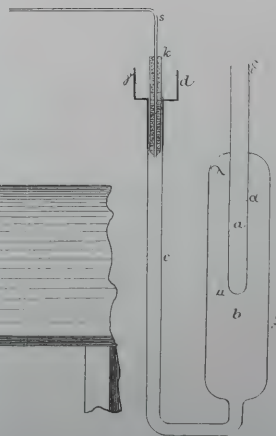


Fig. 1.



specific gravity of water at the same temperature, p the weight (in grammes) of melted ice which corresponds to the volume v (that is, the indication of 1 division of the scale), then

$$\frac{1}{\frac{1}{s_p} + \frac{1}{s_m}} = \frac{p}{v},$$

or

[illegible]

There are numerous observations on the specific gravity of ice ; but how little they agree with each other is shown by the following numbers found for s_s :—

Thomson	0·920
Heinrich	0·905
Osan	0·927
Royer and Dumas . .	0·950
Brunner	0·918
Plücker and Geisler . .	0·920
Kopp	0·908
Dufour	0·922 (maximum)
Dufour	0·914 (minimum)

Owing to the disagreement of these different observers, it seemed to me essential to determine with greater exactness than was possible hitherto the value of the constant p , and therefore also of s_e . I have for this purpose adopted the following plan, by which the sources of errors which have made previous estimations uncertain are completely removed.

Fig. 5 represents a strong U-tube of hard glass which is drawn out at a to a thick point. It is filled with mercury to b_1b , which is well boiled in both limbs as in constructing a barometer. The point a is provided with an india-rubber tube, through which (while the air in the limb ab is slightly warmed and again cooled) distilled water free from air is allowed to pass over the mercury at b . This water is then boiled for half an hour, and the india-rubber tube (c) is kept in a beaker-glass filled also with boiling water; as soon, then, as the boiling is interrupted at b , the space ab becomes filled completely with water quite free from air. The india-rubber tube (c) is then closed under water with a piece of glass rod, and the point at a is sealed up. This is better done with a Bunsen lamp than a blowpipe, by heating the part where the tube covers the drawn-out point so strongly that it is filled with vapour instead of with water. If the apparatus has been weighed before filling with water, and is weighed again

after filling along with the perfectly dry glass point, the weight of the enclosed water is obtained. The open limb is then completely filled with boiled mercury by means of a long capillary glass tube, which prevents air-bubbles adhering to the sides of the tube. If the apparatus be now exposed in the open air to a temperature under 0°C ., an ice-cylinder is formed which at last partly closes up, leaving a thin column of water unfrozen. By freezing this last portion of water the ice already formed is exposed to very great pressure, which sensibly alters its specific gravity; this pressure is so great that it can burst a glass tube able to resist a pressure of 80 atmospheres.

To remove this irregularity, and to allow the formation of ice during the whole experiment to take place under the same pressure, it is simply necessary to surround the whole instrument with sawdust and to expose only the upper part at *a* to an air temperature under 0°C .. After a mass of ice has been formed at *a* by exposure to a low temperature, it is allowed to melt, with the exception of a small portion in the centre; and the freezing then proceeds regularly downwards from *a* to *b*, and can be very conveniently regulated if the limb containing the water is gradually raised as required out of the sawdust. The cylinder of ice forms a half-spherical cavity at its base, which proceeds unchanged till the level of the mercury at *b* is reached and the last portions of water are frozen from above to below. As soon as the formation of ice ceases, the whole apparatus is exposed for some time to a temperature under 0°C ., so as to freeze the last traces of water which are found at *b* between the mercury and the glass. The cylinder of ice thus formed, perfectly free from air-bubbles, resembles the purest crystal for clearness and transparency. By tightly inserting the cork (*e*) in the open limb of the apparatus (A), the tube B is joined to A; and no trace of air must remain between the cork and the mercury, which is forced by the pressure through the capillary tube (*f*) into the vessel (B) containing mercury to the level *g*. The capillary tube is secured with the finest sealing-wax into the cork, which must be smooth and perfectly free from pores. It is quite unnecessary to secure the cork in the wider tube also with sealing-wax, since a displacement of this is as little to be feared as an elastic rebound (*elastische Nachwirkung*); I have convinced myself of this by direct experiments. The apparatus so arranged is placed in a room with as constant a temperature as possible, and surrounded on all sides and over the cork with a thick layer of snow, which has not lain at a temperature above 0° and become impregnated with moisture, but which is dry and coherent.

If the apparatus has assumed a temperature of 0°C . after standing six or twelve hours, the mercury-vessel is removed from the cork (*h*) and weighed with the mercury it contains, and re-

placed again in the cork, care having been taken to remove any mercury adhering to the end of the capillary tube. The apparatus is then removed from the snow and the ice melted by radiation from a Bunsen lamp placed near it, and is again surrounded with snow and allowed to assume a temperature of 0°C . The mercury-vessel is again withdrawn and weighed. The increase of weight from the first weighing is the weight of mercury which represents the diminution of volume, calculated for 0°C ., which the ice cylinder at the temperature of 0°C . has suffered by its melting to water at 0°C .

Let G_w be the weight of frozen water;

G_q the weight of mercury driven out by the melting of the ice;

s_w the specific gravity of the water at 0°C .;

s_q the specific gravity of the mercury at 0°C .;

s_e the specific gravity of ice at 0°C ;

then

$$\frac{G_q}{s_q} + \frac{G_w}{s_w} = \frac{G_w}{s_e};$$

therefore

$$\frac{s_w G_w}{G_w + \frac{s_w}{s_q} G_q} = s_e.$$

Owing to the great exactness of which this method is capable, it seemed to me unnecessary to make more than three experiments. In the first experiment the water froze between -3°C . and -5°C ., in the second between -1°C . and -3°C ., in the third between 0°C . and -2°C . The following weights (calculated for *in vacuo*) were found:—

$$G_w = 14.1580 \text{ grms.}$$

$$\text{Experiment 1. . } G_q = 17.4400 \text{ ,,}$$

$$\text{Experiment 2. . } G_q = 17.4624 \text{ ,,}$$

$$\text{Experiment 3. . } G_q = 17.4757 \text{ ,,}$$

further,

$$s_w = 0.99988$$

$$s_q = 13.59600;$$

then the specific gravity of ice s_e is

$$\text{Experiment 1. . . } 0.91682$$

$$\text{Experiment 2. . . } 0.91673$$

$$\text{Experiment 3. . . } 0.91667$$

$$\text{Mean of the three experiments } 0.91674$$

For the magnitudes in equation (2),

$$p = \frac{v s_e s_w}{s_w - s_e},$$

we have therefore the values:—

$$v = 0.00007733,$$

$$s_e = 0.91674,$$

$$s_w = 0.99988;$$

and thence

$$p = 0.00085257.$$

The weight of melted ice (e) in grammes corresponding to T corrected divisions on the scale is therefore

$$e = 0.00085257T. \quad . \quad . \quad . \quad . \quad . \quad (3)$$

If the latent heat of liquidity of water be called l , a scale-division pl corresponds to the unit of heat above defined. For the amount of heat w , expressed in units of heat, which T scale-divisions indicate, we have

$$w = plT;$$

or when we take for l the value 80.025 as found below, then

$$w = 0.068227T. \quad . \quad . \quad . \quad . \quad . \quad (4)$$

Since the ice-cylinder surrounding the vessel a weighs from 40 to 50 grms., and on an average in each experiment only about 0.35 gm. of ice requires to be melted, which corresponds to somewhat more than four hundred scale-divisions, it is possible, with the same ice-cylinder, to make one hundred calorimetric researches and to use the same apparatus (prepared once for all) for a week, if only care is taken to renew the snow round the instrument every night and morning. The ice cylinder is easily produced by an arrangement represented in fig. 2. A is a tin-plate vessel containing alcohol, and B an empty tin-plate vessel, which are both cooled down in a freezing-mixture of salt and snow to about -20° C. C represents the inner vessel (a , fig. 1) around which the ice-cylinder is produced. On sucking at the tube a , the cold alcohol of the vessel A is brought over into the vessel B through the vessel C ; and therefore, conversely, on sucking at b the alcohol is brought back into the vessel A through C . By alternate sucking at a and b , the vessel C can, to the level α , by means of fresh cold alcohol be kept at a temperature of from -10° C. to -15° C., and thus the ice cylinder gradually formed in the mass of water (b , fig. 1) surrounding the tube C .

I have constructed this ice-producing apparatus in the form shown at fig. 3. The *two* semicylindrical tin-plate vessels *a* and *b* correspond to the *single* vessel A shown in fig. 2, and are connected by tubes with each other and with the tube *a*; while the similar tin-plate vessel, the outside of which is marked *c*, corresponds to the vessel B, fig. 2. These two vessels, forming two concentric chambers round the tube α_1 , possess a great cooling-surface, and are both immersed in the same freezing-mixture. The arrangement of tubes in fig. 3, by which the circulation of the cooled alcohol is accomplished, is easily understood, as the corresponding caoutchouc tubes are denoted by the same letters as in fig. 2. The alternate suction of the alcohol is managed by means of the stopcock H, which is connected at *w* with the water-pump. When this stopcock is placed in one position, the tube *q* communicates with the exhausting-tube *w*, and the tube *p* with the outer air; when placed in the other position, the reverse takes place, and *w* communicates with *p* and *q* with the outer air. The production of the cylinder of ice becomes a very simple operation by this arrangement. The cooling-apparatus with its caoutchouc tubes is placed in the freezing-mixture, *p* and *q* are connected with the stopcock H, and *w* with the water-pump; the caoutchouc stopper with the tubes *m*, *n* at C is sunk in the inner vessel of the instrument, and, lastly, the tubes *m* and *n* are connected with the corresponding glass tubes of the cooling-apparatus. If, after the stopcock of the water-pump has been opened, the reversing stopcock is turned alternately, the stream of cooled alcohol can be kept at work as long as necessary for the production of the cylinder of ice. The formation of this last is easily observed with the naked eye or with a telescope, and presents some not uninteresting peculiarities. The temperature of the air-freed water in the outer vessel (*b*, fig. 1) sinks by degrees (without freezing taking place) far below 0°C ., while the outside of the vessel becomes covered with a coating of ice formed from atmospheric moisture, which even vigorous shaking cannot remove. At last, when the temperature has sunk very low, the formation of ice begins suddenly, and spreads in a few seconds from λ to μ . The whole vessel down to these limits is filled with thin plates and needles of ice; but the water from μ to the level of the mercury at β is not frozen. By continued cooling the formation of the ice-cylinder now begins, and is allowed to go on until it has attained a thickness of from 6 to 10 millims. This shell of ice below μ appears completely amorphous, as clear and transparent as the purest crystal; the upper part between μ and λ , however, is not transparent, and has a coarsely fibrous texture. When, however, the instrument has stood in snow for some days at 0°C ., this coarse

fibrous texture entirely disappears. The ice between λ and μ now consists of smooth rounded transparent grains. If the instrument, after continued use, is exposed to the heat of the room, the single grains melt on their surface, and thereby free themselves from each other and ascend in the liquid, and in doing so occasionally appear like globules of the yeast plant strung together. In order, then, to understand more clearly the formation of ice, so far as the use of the instrument is concerned, the calorimeter, with its ice-cylinder carefully surrounded by snow, was observed for a considerable time. The instrument in this and in all the following experiments was placed in a large earthenware decanting-jar, from the lower aperture in which the water formed by the melting of the snow could constantly flow out; in this way contact of the lower part of the instrument with the water was avoided. The instrument soon becomes surrounded with a compact mass of congealed snow (*Firneis*). If after twelve or fifteen hours, owing to external melting of this ice, a considerable space is formed at the sides of the jar, this space is increased by cutting away with a wooden spatula the loosely caked *Firneis*, which is pushed below the calorimeter and its place supplied by fresh snow. The first experiment was made with fresh-fallen snow which was perfectly free from earthy impurities. Some hundredweights of this snow were preserved in a clean wooden box as a supply for refilling during the experiments. With such a supply the calorimeter can be kept working for a whole week, and refilled twice daily without the ice-cylinder needing to be renewed. During the whole length of the observations, which took five days, the inner vessel, surrounded by a cylinder of ice, was closed with an india-rubber stopper, and the whole instrument, with the exception of the scale, enclosed on every side with melting snow. The temperature of the room in which the experiments were made varied from $0^{\circ}5$ C. to 6° C. The experiments are collected in Table I. Column I. contains the times of the observations in hours; column II. gives the readings of the calorimeter-scale during the same times; those marked with asterisks are the observed values, from which the remaining ones are calculated by interpolation. The mercury which escaped from the scale-tube was weighed up to the thirty-first hour, and the weights found converted into divisions of the scale by means of equation (1). Column III. is calculated by means of equation (3), and represents the weight in grammes of the ice formed in the instrument from the commencement of the time.

TABLE I.

I.	II.	III.	I.	II.	III.	I.	II.	III.
0.	0.0	0.0	41.	2739.8	2.3358	82.	2810.5	2.3961
1.	633.8	0.5404	42.	2740.3	2.3363	83.	2811.6	2.3971
2.	1267.6	1.0807	43.	2740.8	2.3367	84.	2812.8	2.3981
3.	1901.4*	1.6211	44.	2741.3	2.3371	85.	2813.9	2.3990
4.	2033.1	1.7334	45.	2741.8*	2.3376	86.	2815.0	2.4000
5.	2164.8	1.8456	46.	2743.3*	2.3389	87.	2816.1	2.4009
6.	2296.5	1.9579	47.	2744.7	2.3401	88.	2817.2	2.4018
7.	2428.2*	2.0702	48.	2746.2*	2.3413	89.	2818.4	2.4029
8.	2443.4	2.0832	49.	2747.0	2.3420	90.	2819.5	2.4038
9.	2458.6	2.0961	50.	2747.8	2.3427	91.	2820.6*	2.4048
10.	2473.8	2.1091	51.	2748.6	2.3434	92.	2822.2	2.4061
11.	2489.0	2.1220	52.	2749.4	2.3441	93.	2823.9*	2.4076
12.	2504.2*	2.1350	53.	2750.3*	2.3448	94.	2825.1	2.4086
13.	2528.6	2.1558	54.	2751.6*	2.3460	95.	2826.4	2.4097
14.	2553.0	2.1766	55.	2753.0*	2.3471	96.	2827.6*	2.4107
15.	2577.4	2.1974	56.	2754.4	2.3483	97.	2828.9	2.4118
16.	2601.8	2.2182	57.	2755.8*	2.3495	98.	2830.2	2.4130
17.	2626.2	2.2390	58.	2758.0	2.3514	99.	2831.6	2.4141
18.	2650.6	2.2598	59.	2760.2	2.3533	100.	2832.9*	2.4152
19.	2675.0	2.2806	60.	2762.4	2.3551	101.	2834.2	2.4164
20.	2699.4	2.3014	61.	2764.6	2.3570	102.	2835.6	2.4176
21.	2723.8*	2.3222	62.	2766.9	2.3590	103.	2836.9*	2.4186
22.	2724.8	2.3231	63.	2769.1	2.3608	104.	2837.6	2.4193
23.	2725.8	2.3239	64.	2771.3	2.3627	105.	2838.3	2.4199
24.	2726.8	2.3248	65.	2773.5	2.3591	106.	2839.0	2.4204
25.	2727.8	2.3256	66.	2775.7	2.3665	107.	2839.7	2.4211
26.	2728.8	2.3265	67.	2777.9	2.3684	108.	2840.4	2.4217
27.	2729.8	2.3273	68.	2780.9	2.3709	109.	2841.1	2.4223
28.	2730.8	2.3282	69.	2783.9*	2.3735	110.	2841.8	2.4229
29.	2731.8	2.3291	70.	2786.2	2.3754	111.	2842.5	2.4234
30.	2732.8	2.3297	71.	2788.5*	2.3774	112.	2843.2*	2.4240
31.	2733.8	2.3307	72.	2790.7	2.3793	113.	2843.9	2.4246
32.	2734.8	2.3316	73.	2792.9	2.3811	114.	2844.6*	2.4252
33.	2735.8*	2.3324	74.	2795.0	2.3829	115.	2844.6	2.4252
34.	2736.3	2.3329	75.	2797.1	2.3847	116.	2844.6*	2.4252
35.	2736.8	2.3333	76.	2799.2	2.3865	117.	2844.6	2.4252
36.	2737.3	2.3337	77.	2801.3*	2.3883	118.	2844.6*	2.4252
37.	2737.7	2.3342	78.	2803.2	2.3899	119.	2844.6	2.4252
38.	2738.3	2.3346	79.	2805.2*	2.3916	120.	2844.6*	2.4252
39.	2738.8	2.3350	80.	2807.3	2.3934			
40.	2739.3	2.3354	81.	2809.4*	2.3952			

This Table shows:—first, that about 2 grammes of the water contained in the calorimeter were frozen at the temperature of melting snow during the first seven hours; secondly, that this freezing at the temperature of the melting snow continued for 114 hours in a decreasing ratio; and, lastly, that after this length of time a period arrived when, at the temperature of the melting snow, the water ceased to freeze. The disproportionate quantity of ice formed at the beginning of the experiment clearly depends on the low temperature of the ice-cylinder at first, it

having been formed at from -15° to -20° , as the following consideration shows:—If we assume that the quantity of ice (g) formed within the first seven hours has been caused by the loss of heat which the water suffers in heating the ice cylinder from $-t^{\circ}$ to 0° , then the mean temperature which the ice cylinder must have had in order to produce that weight of ice is obtained from the equation

$$t = -\frac{lg}{s_w G}.$$

In this equation l stands for the heat of liquidity of water, s_w for the specific heat of ice, and G for the weight of the ice cylinder after cooling to t° . In this equation G only is unknown. In order to determine it, the open end of the scale-tube was dipped, at the conclusion of the series of experiments, Table I., into a weighed vessel containing mercury; and after the melting of the ice cylinder, and when the instrument had been brought again to 0° , the loss of weight G_1 of the mercury-vessel was determined. The weight sought is

$$G = \frac{G_1 p}{s_q v},$$

in which s_q represents the specific weight of mercury at 0° C., p the weight of melted ice which corresponds to one division of the instrument (equation 3), v the volume of one scale-division (equation 1). The values of the quantities in this and in the preceding equation are

$$l = 80.03,$$

$$s_q = 13.596,$$

$$s_w = 0.48,$$

$$g = 2.13 \text{ grms.}$$

$$G = 61.227 \text{ grms.,}$$

$$p = 0.0008526 \text{ grm.,}$$

$$v_1 = 0.00007733 \text{ cub. cent.}$$

By substituting the same in the equations, we obtain for the weight of the ice cylinder used in the observations

$$G = 49.65 \text{ grms.,}$$

and for the temperature

$$t = -6.95 \text{ C.}$$

Therefore the ice cylinder formed at a temperature of at least -15° C. only needed to have a temperature of -7° C. in

order to cause by its heating to 0°C . the formation of ice found by the observations in the first seven hours.

Since it can be concluded from the determination of specific heats given below that a time of seven hours is more than sufficient to equalize a difference of temperature of 7°C . in the instrument, the formation of ice, which (as is seen from Table I.) has lasted more than 100 hours, must be ascribed to some other cause than that considered. Without entering into the question whether (as C. Schultz* assumes) this cause be due to the air contained in the snow-water, or whether the transformation of snow into *Firneis* play a part in it, let it suffice at present to consider only some of the causes which produce a lowering of the melting-point, which deserve particular consideration in using the ice-calorimeter. If the pure snow surrounding the instrument be soaked with just so much boiled water at 0°C ., or distilled water at the same temperature which has been first shaken up with air, as will remain in it, no freezing will take place, at all events, during the first twelve hours (the observations were continued no longer); but such a melting of the ice will take place as to render the instrument under these circumstances totally useless for observations. On the other hand, the slightest impurity in the snow produces such a constant deposition of ice on the ice cylinder, as to cause the mercury-thread to move many divisions of the scale in a minute. Snow which contains only traces of salts from the ground, or which has absorbed animal or vegetable impurities from the street-pavement, shows this lowering of the melting-point in the most striking manner. River-ice, so pure that water melted from it produced scarcely a faint turbidity in solutions of barium and silver, produced in the instrument in three days 2 grms. of ice. After these observations it is clear that only the purest snow can be used in these experiments. It is also advantageous to experiment in a room whose temperature is not much above 0°C ., and not to begin the observations until the formation of ice on the ice cylinder does not cause an alteration of more than a few divisions of the scale in an hour. But, above all, it is necessary to see that, before the instrument has assumed a constant temperature in the snow, a small layer of water is formed by melting between the glass sides and the adjacent cylinder of ice, in order to avoid unequal tension and consequent elastic rebound (*elastische Nachwirkung*).

The accuracy of the results, however, depends most essentially on the care with which absorbed air is excluded from the water and mercury when the instrument is first prepared. This is managed in the following way. The instrument half-filled with boiled water is inverted, mouth downwards, and secured with a clamp.

* Poggendorff's *Annalen*, vol. cxxxvii. p. 253.

The mouth of the tube *c*, fig. 1, which is not yet provided with the iron collar *d*, is then plunged in a beaker containing water kept boiling; and the water in the instrument, reaching to the level β , is boiled down to one-third of its original bulk. On removing the lamp used in heating the apparatus, it fills itself with water free from air. It is then allowed to cool, placed upright as in the drawing, and filled up to the level β with so much freshly boiled mercury that the level of mercury in the vessel *b* and in the tube *c* is nearly the same height. The water is now for the most part removed from the tube *c* by means of a siphon, and the tube freed from moisture by means of a dry current of air produced by the water-pump; and now, for the first time, the iron collar *d* is fastened on with the finest sealing-wax so as to leave a small part of the tube *c* projecting above the inner bottom of *d*, so that the cork of the scale may afterwards be placed in the tube *c* itself and not in the iron collar. The final filling-in of boiled mercury to the level γ is done with a hollow capillary glass tube, so as to avoid any air-bubbles remaining on the sides of the tube.

In order to place the mercury-thread at the same starting-point on the scale in each experiment, it suffices to press the cork of the scale-tube a little deeper into the mouth of the mercury-tube *c*, fig. 1. If in doing this the mercury-thread goes beyond the desired mark, a brass weight tied to a thread is warmed in the hand or under the tongue and dropped into the fluid *a*, fig. 1. Suppose the brass weight weigh *g* grms., and suppose its temperature is denoted by *t*, its specific heat by *s_m*, the latent heat of water by *l*, and if we denote by *p* the weight of melted ice (found by equation 2) which the indication of one scale-division represents, then the variation caused by the brass weight will be

$$\frac{s_m t g}{l p}$$

divisions of the scale.

If we take

$$t = 37^\circ \text{ C.},$$

$$s_m = 0.0939,$$

$$l = 80.03,$$

$$p = 0.000853,$$

and for *g* the series 0.1, 0.2, 0.4, 0.6 . . . grm., then we obtain for these weights in round numbers the following retrogression of the mercury-thread:—

0.1	gram.	5	divisions of the scale,
0.2	"	10	" "
0.4	"	20	" "
0.6	"	30	" "
0.8	"	40	" "
1.0	"	50	" "

After one of these weights, heated under the tongue, has been dipped in, the mercury-thread can be allowed to go back to the desired mark. The great alteration on the scale caused by introducing a weight only warmed to 37° C. is quite sufficient to indicate the extraordinary sensitiveness of the instrument. The elevation of temperature which 0.4 gram. brass at 37° C. caused on dipping into about 20 grms. of water was found by the thermometer to be only $0^{\circ}07$ C.; while with the calorimeter just described the variation amounted to 20 divisions of the scale, each of which, in the instrument used, measured a millimetre.

In reference to reading off on the scale, we have still to remark that before each observation with the telescope it is necessary (especially when the scale-tube is very narrow) to shake it gently until the capillary resistance is overcome, and the mercury-thread by further shaking does not alter its position.

Table I. shows that the mercury-thread of the instrument was not completely stationary. The alteration, which may amount from 1 to 3 divisions in an hour, either in a positive or negative direction, is nearly proportional to the time, as is easily proved by using the calorimeter. The small error thus caused is removed in the following way:—As soon as the observer sees that the instrument has become stationary enough, he notes from half-hour to half-hour the position of the mercury-thread. If the alteration of the mercury amounts in m_0 minutes to τ_0 scale-divisions, then the deviation caused by disturbing influences is for one minute

$$\frac{\tau_0}{m_0}.$$

The time M_0 and the position of the mercury-thread Q_0 are now observed at the moment when the substance to be examined is taken from the heating-vessel f , fig. 4, and dropped into the calorimeter-vessel a , fig. 1. Both observations are repeated an hour later, and M_1 and Q_1 thus obtained, and lastly, once more, as at the beginning of the experiment, the alteration of the mercury-thread which is independent of the heat to be measured,

$$\frac{\tau_1}{m}.$$

The mean independent alteration of the mercury-thread in the experiment amounted in a minute to

$$\frac{1}{2} \left(\frac{\tau_0}{m_0} + \frac{\tau_1}{m_1} \right),$$

and during the entire duration of the experiment,

$$(M_1 - M_0) \frac{1}{2} \left(\frac{\tau_0}{m_0} + \frac{\tau_1}{m_1} \right)$$

divisions of the scale. This value is to be added as a correction to the indication of the mercury-thread $Q_0 - Q_1$ observed in the experiment, and the negative sign used whenever, independent of the experiment, a melting of ice was indicated; in the opposite case a positive sign must be used.

For the indication T corresponding to the amount of heat measured, we obtain therefore the equation

$$T = (Q_0 - Q_1) + (M_1 - M_0) \frac{1}{2} \left(\frac{\tau_0}{m_0} + \frac{\tau_1}{m_1} \right); \quad . \quad . \quad (5)$$

in which we need scarcely remark that the values of the table of calibration are to be substituted for the direct readings to which they correspond.

2. *Estimation of Specific Heat.*

In order to obtain the specific heat of a substance, it is best to estimate once for all, in divisions of the scale, the amount of heat given up by one gramme of water when it is cooled from 1°C. to 0°C. ; we have then to divide by this value, W_w , the amount of heat W which a grain of the substance to be examined loses under the same circumstances. Suppose the weight of substance to be G , its temperature t , the number of corrected divisions of the scale T , then we obtain the desired specific heat from the equation

$$S = \frac{T}{W_w G_t}, \quad . \quad . \quad . \quad . \quad . \quad (6)$$

in which t represents the boiling-point of water at the barometric pressure which existed during the experiment.

The apparatus (fig. 4) is employed to impart to substances the constant temperature t ; and it is represented one-sixth of its real size. The tin-plate vessel A , provided with a water-gauge, holds enough water to produce a current of steam for twelve hours when heated by the gas-lamp underneath. The current of steam passes through the india-rubber tube a and the outer glass vessel (B) into the second india-rubber tube, b , which is connected with an ordinary condenser. The heating-vessel (f) is placed in the centre of B and is thus constantly surrounded with the

current of steam; it is in the form of a common thin glass test-tube, the top of which, however, is slightly narrowed and sharply cut off. The mouth projects some millimetres above the india-rubber stopper which closes the outer vessel (B). When the substance has been heated for about an hour in the vessel (*f*), the whole apparatus (which is supported on the ring of cork, *n*), with the mouth (*p*) still closed and the steam still passing through, is brought near the similarly closed tube (δ , fig. 1) of the calorimeter surrounded with snow. The stopper of the vessel *f* is quickly withdrawn, and at the same moment the vessel B is inverted and the heated substance thus allowed to drop into the water (*a*, fig. 1). The time occupied in falling amounts only to a very small fraction of a second, so that the cooling during this time is too small to be considered.

At the bottom of the inner vessel (*a*, fig. 1) a small loose and thoroughly moistened piece of cotton-wool is placed, which is prevented from rising up by being wound round a piece of platinum wire. This cotton-wool serves two purposes: first, it prevents the breaking of the tube when heavy bodies are dropped into it; and, secondly, it serves to remove the substances which have been examined from the apparatus. For this purpose a platinum wire suitably bent at the point is pushed into the cotton-wool, which by this means is drawn up to the mouth of the tube; the substance lying on it is then removed, and the cotton-wool, without ever being removed from the tube, is restored to its original place.

The chief advantage which the instrument described possesses over all other calorimetric arrangements, not to mention its great sensitiveness, consists in the fact that all the heat which the body gives up is employed to melt ice. The weight of substance which is placed in the water of the vessel *a*, fig. 1, at 0° C. is so small compared with the weight of the water itself, that the temperature never reaches 4° C. Since water at this temperature possesses its maximum density, the heated liquid at the bottom of the vessel *a* can never rise, and is protected from the loss of heat not needed for the melting of the ice by a column of water lying above it at a temperature of 0° C. whose power of conducting heat is indefinitely small.

This process is very nicely seen in the ice cylinder when it has been used for thirty or forty determinations, when there is found, quite at the bottom of the vessel *a*, a hollow space filled with water, which very generally has the form of a little digesting-flask, while the ice cylinder in the whole space above appears quite unaffected. The weight of the substance to be examined need not exceed 0.3 or at most 4 grms., according to the greatness of the specific heat to be expected. If the substance is fluid,

or if it is affected by contact with air or water, it is enclosed just as in organic analyses, in as light a glass tube as possible, whose weight is noted along with that of the substance. If the substance floats on water, either by itself or when enclosed in a glass tube, the latter is loaded with a spiral of platinum wire, also weighed, and heavy enough to sink it. Perhaps it would be still more convenient to use a light and well-stoppered platinum vessel. The amount of heat which the glass tube and platinum spiral give up are calculated as follows. If we call G_g the weight of the glass tube, t its temperature, and W_g the amount of heat measured in divisions of the scale which 1 gm. of the same glass as the tube gives up on cooling from 1°C. to 0°C. , and if we denote the same quantities for platinum by G_p , t , and W_p , then we obtain the equation

$$S = \frac{\frac{T}{t} - (W_g G_g + W_p G_p)}{W_g G} \dots \dots \dots (7)$$

Next, the constants W_g , W_p , and W_w have to be determined once for all. W_p is found by the equation

$$W_p = \frac{T}{Gt}.$$

From the following observations, in which G represents the weight of platinum used in the experiment,

$$T = 216.6,$$

$$t = 100.0^\circ \text{C.},$$

$$G = 4.5942 \text{ grms.};$$

therefore

$$W_p = 0.4692.$$

W_g was calculated by the same equation from the following observations with two kinds of glass, in which G represents the weight of glass used in the experiment, and the data for the determination of T are given by equation (5).

TABLE II.

		Glass I.	Glass II.	
		Experiment I.	Exp. 2.	Exp. 3.
Weight of glass	G	0.5706 grm.	1.0114 grm.	1.4319 grm.
Temperature of glass.....	t	99.74°C.	99.474°C.	99.474°C.
Duration of experiment....	$M_1 - M_0$	29'	87'	56'
Scale before experiment ..	$\frac{\tau_0}{m_0}$	-0.1	0.0	+0.005
Scale after experiment ...	$\frac{\tau_1}{m_1}$	-0.1	0.0	+0.014
Indication of the scale ...	$Q_0 - Q_1$	159.1	287.9	410.4

For the first glass,

$$W_g = 2.7446;$$

for the second,

$$W_g = 2.8616$$

$$W_g = 2.8777$$

$$\text{Mean} \quad . \quad . \quad = 2.8697$$

W_w is obtained from the following two experiments by the equation

$$W_w = \frac{\frac{T}{t} - (W_g G_g + W_p G_p)}{G}.$$

TABLE III.

		Experiment I.	Experiment II.
Weight of water	G	0.3333	0.3333
Weight of glass tube	G_g	0.2223	0.2223
Weight of platinum sinker ...	G_p	0.5230	0.5230
Temperature of heating	t	99°.474 C.	99°.474 C.
Duration of experiment.....	$M_1 - M_0$	76'	86'
Scale before experiment	$\frac{\tau_0}{m_0}$	0.0	+0.067
Scale after experiment	$\frac{\tau_1}{m_1}$	0.0	+0.064
Observed scale-divisions	$Q_0 - Q_1$	573.9	568.1
Constants	$W_p = 0.4692; W_g = 2.870$		

The calculation gives :—

$$\text{From Experiment 1.} \quad . \quad W_w = 14.660$$

$$\text{From Experiment 2.} \quad . \quad W_w = 14.654$$

$$\text{Mean} \quad . \quad . \quad W_w = 14.657$$

In illustration of the method described, I now give the results of determinations made with chemically pure substances, whose specific heat has been determined with great exactness by the methods up till now in use. The details of these observations are arranged in the following Table IV., in which the letters used refer to those employed in equations (5) and (7) :—

TABLE IV.

Substance examined		Cast silver.	Cast zinc.	Cast antimony.	Cast cadmium.	Very old roll sulphur.
Weight of substance	G	3.6320	2.5150	3.8575	1.8675	1.0708
Weight of glass tube	G_g	0	0	0	0	0
Weight of platinum sinker ..	G_p	0	0	0	0	0
Temperature of heating	t	100°·00 C.	99°·80 C.	99°·80 C.	99°·80 C.	100°·00 C.
Duration of experiment	$M_1 - M_0$	60'	60'	64'	63'	55'
Scale before experiment	$\frac{\tau_0}{m_0}$	0	0	0	0	0
Scale after experiment	$\frac{\tau_1}{m_1}$	0	0	0	0	0
Indication of the scale	$Q_0 - Q_1$	297.7	343.8	279.5	146.7	268.8
Constants	W = 14.657					

In the following Table V. the specific heats calculated from these data are placed beside those of Regnault obtained by the mixture method:—

TABLE V.

Substances.	Ice-calorimeter. a .	Regnault's results, b .	$a - b$.
Water	1.0000	1.0000	—
Silver	0.0559	0.0570	—0.0011
Zinc	0.0935	0.0956	—0.0021
Antimony	0.0495	0.0508	—0.0014
Cadmium	0.0548	0.0567	—0.0019
Sulphur	0.1712	0.1764	—0.0052

Thus the values obtained by the ice-calorimeter are seen to agree very nearly with those obtained by Regnault, but are always a little less. Whether or not these constant deviations are due to differences in the methods adopted can scarcely be decided from so few experiments, especially as these were made while I was engaged with other work; but still, although the were not made with very special care, they were free from any considerable source of error.

The following Table VI. contains experiments with several pure elements whose specific heats have not been determined hitherto. The specific and atomic heats calculated from these results are arranged together in Table VII. :—

TABLE VI.

Substance ex- amined		Indium.	Indium.	Ruthe- nium.	Cal- cium.	Cal- cium.	Allo- tropic tin.	Cast tin.
Weight of sub- stance	G	1.1514	1.1514	1.7927	0.2823	0.2823	2.2394	3.0017
Weight of glass tube	G _g	0	0	0.3287	0.6683	0.6683	0.3953	0
Weight of plat- inum sinker ...	G _p	0	0	0.4239	0.4239	0.4239	0	0
Temperature of heating	t	99°.82C.	99°.82C.	99°.60C.	99°.78C.	99°.78C.	99°.786C.	99°.606C.
Duration of ex- periment	M ₁ - M ₀	46'	47'	106'	65'	76'	66'	65'
Scale before ex- periment	$\frac{\tau_0}{m_0}$	-0.130	-0.063	-0.160	-0.090	-0.052	-0.120	0.090
Scale after ex- periment	$\frac{\tau_1}{m_1}$	-0.020	-0.037	-0.110	-0.130	-0.076	-0.16	0.13
Indication of the scale	Q ₀ - Q ₁	100.2	97.5	276.8	280.2	277.3	296.0	252.1
Constants	W _g =2.745; W _p =0.4692; W _w =14.657.							

TABLE VII.

Elements.	Specific heat. a.	Atomic weight. b.	Atomic heat. a × b.
Ruthenium	0.0611	52.0	3.18
Calcium	0.1722	20.0	3.44
Calcium	0.1686	20.0	3.37
Allotropic tin.....	0.0545	58.8	3.21
Cast tin	0.0559	58.8	3.39
Indium	0.0574	37.8	2.17
Indium	0.0565	37.8	2.33

We must make the following remarks with regard to the materials used, and the results obtained, in these experiments. The ruthenium was prepared from the material known as "iron residues" of the St. Petersburg mint, which is perfectly free from osmium. This material was heated with chloride of barium in a current of chlorine, and the grey powder obtained was fused with potash, and a considerable quantity of ruthenate of potash obtained. The oxide prepared from this salt by fractional precipitation with carbonic acid was converted by hydrochloric acid into chloride, the aqueous solution of which was precipitated by hydrogen, and the metal so obtained in the form of shining scales was freed from every trace of oxide by heating it in a current of hydrogen. It was found by testing free from all other platinum-metals. As was to be expected, the specific heat found agrees

with the atomic weight hitherto accepted. The calcium was prepared by electrolysis from pure chloride of calcium. It formed small pale golden-yellow globules of bright metallic lustre, which speedily become grey on exposure to the air. Before enclosing it in the glass vessel, it was scraped bright in an atmosphere of dry carbonic acid. On testing it was found to be nearly perfectly pure. From the specific heat found, it may be concluded that the atomic weight hitherto accepted, $\text{Ca}=20$, is correct, and must not be halved, as has become necessary in the case of the alkaline metals.

By allotropic tin is meant that peculiar modification of the metal which appears to be formed from ordinary tin exposed for a length of time to very low temperatures. The piece used for experiment was obtained from the large mass transformed during an unusually long and severe winter, in which Fritsche observed this remarkable allotropism, first described by him. The mass consisted of an aggregation of angular stalks loosely coherent in one direction, and crumbling by slight pressure. This tin, as I convinced myself, is of a high degree of purity, is free from any trace of arsenic or antimony, and dissolves entirely in trisulphide of potassium without any residue of metallic sulphides. The little stalks of which it is composed are not brittle, but ductile like ordinary tin. The specimen of ordinary tin examined was obtained by melting the allotropic metal. Both modifications possess nearly the same specific gravity.

The indium used appeared perfectly free from tin, cadmium, and iron. After oxidation by nitric acid and evaporation with sulphuric acid, it left no trace of sulphate of lead when dissolved in alcohol. 1.0592 grm. of the metal was dissolved without loss in nitric acid, and yielded on evaporation and ignition 1.2825 grm. oxide of indium. If we regard the latter, according to the hitherto accepted view, as consisting of equal atoms, the value of the atom of indium from this determination is

$$\text{In} = 37.92;$$

which figure agrees very nearly with Winkler's determination,

$$\text{In} = 37.81.$$

This atomic weight, multiplied by the specific heat s_c , gives for the atomic heat of indium the value

$$s_c \text{In} = 2.13,$$

which does not agree with the other ones. Therefore the hypothesis that the formula of oxide of indium is InO appears no longer tenable. If the atomic weight be taken as once and a half the above,

$$\text{In} = 56.7;$$

the atomic heat becomes

$$s_c \text{In} = 3.23,$$

which is nearly equal to that of the other elements. The atomic weight 56.7 removes the anomaly produced by considering the oxide of indium as a monoxide, and ranks it in its whole behaviour with the trioxides which do not form alums. Therefore the formulæ of column II. Table VIII. must be substituted for those hitherto accepted in column I.

TABLE VIII.

	I.	II.
Black indous oxide	$\text{In}^2\text{O}?$	$\text{InO}?$
Yellow indic oxide.....	InO	In^2O^3
Green oxide of indium	$5\text{InO}, \text{In}^2\text{O}$	$2\text{InO}, \text{In}^2\text{O}^3$
Grey oxide of indium	$4\text{InO}, \text{In}^2\text{O}$	$3\text{InO}, 2\text{In}^2\text{O}^3$
Indic hydrate.....	InO, HO	$\text{In}^2\text{O}^3, 3\text{HO}$
Indic sulphate	$\text{InO}, \text{SO}^3, 3\text{HO}$	$\text{In}^2\text{O}^3, 3\text{SO}^3, 2\text{HO}$
Indic chloride	In Cl	In^2Cl^3
Ammoniochloride of indium...	$2\text{NH}^4\text{Cl}, 3\text{InCl}, 2\text{HO}$	$2\text{NH}^4\text{Cl}, \text{In}^2\text{Cl}^3, 2\text{HO}$

The ammoniochloride of indium, prepared by R. G. Meyer, has, according to the new formulæ, a composition analogous to the ammoniochloride of rhodium, which also contains 2 atoms of water. I have not yet been able to investigate whether or not the modified atomic weight is confirmed by the isomerism of these salts, but I shall return to it in an investigation on rhodium.

I have not yet been able to ascertain the fitness of the instrument for determining the latent heat of liquidity, as the present winter has been very unsuitable for such experiments. I will only remark that the latent heat of liquidity of water can already be derived from the experiments given in this communication with an exactness which leaves nothing to be desired. According to equation (2), one division of the calorimeter-scale corresponds to

$$\frac{v s_e s_w}{s_w - s_e} \text{ grms.}$$

of melted ice. The constant W_w derived from equation (8) gives the number of divisions of the scale corresponding to one of the units of heat defined at the beginning of this paper. Therefore a division of the calorimeter-scale corresponds to

$$\frac{1}{W_w}$$

units of heat. Let l represent the latent heat of liquidity of water, then

$$\frac{1}{lW_w}$$

gives the weight in grammes of the melted ice which corresponds to one division of the scale. The equation for l is therefore as follows :

$$l = \frac{s_w - s_e v}{W_w s_e s_w v}$$

or, according to equation (2),

$$l = \frac{1}{W_w p}$$

By substituting the values found above,

$$p = 0.00085257,$$

$$W_w = 14.660,$$

$$W_w = 14.654 ;$$

in the equation, therefore, we get for the latent heat of liquidity of water l ,

$$\begin{array}{r} 80.01 \\ 80.04 \\ \hline \text{Mean} \quad . \quad . \quad 80.025 \end{array}$$

The value found by other experimenters by the method of mixture is

According to Regnault	. 79.4
„ Person	. . 80.0
„ Hess	. . . 80.3

It will be possible to determine the heat of combustion of gases by the ice-calorimeter with much greater accuracy than could be done by the method hitherto available. It follows, by equation (4), from the heat of combustion of hydrogen, that 10 cubic centims. of this gas at 0° C. and 760 millims. pressure would produce by its combustion with oxygen an indication of 453 divisions of the scale in the calorimeter of the instrument described. It suffices, therefore, to burn very small quantities of gases (easily prepared pure when so little is required), and to measure the difference produced on the scale, in order to obtain directly the heat of combustion in thermal units, without introducing any of the very uncertain corrections hitherto unavoidable.

XXV. *On some recent Investigations relative to Lunar Activity.**By W. R. BIRT, F.R.A.S.**

[With Two Plates.]

DURING the last seven years the subject of "Lunar Activity," or the present existence of eruptive action productive of "change" on the moon's surface, has been much agitated. In the summer of 1864 my attention was more particularly directed to it, and I suggested that a rigid and careful examination of the moon's disk should be made, the result being embodied in "a catalogue of lunar objects," as all previous records were insufficient to determine the question. This, of course, must be a long and laborious work; but until it is accomplished, at all events for a portion of the moon's surface, it is utterly impossible to decide if an object not previously observed be really "new," or if one presenting a different aspect has undergone a change. This state of affairs was very prominently brought out in the celebrated case of "Linné;" the evidence was clearly insufficient to establish that a change had really taken place. It was not, however, the vagueness or imperfection of the evidence collected in 1866, 1867, and 1868 that occasioned the failure, but the want of precision which characterized the earlier observations, and the doubt resting upon the accuracy of the earlier drawings and descriptions, the object having been lost sight of during a period of twenty-four years between 1842 and 1866. This doubt still continues.

In February 1869, Mr. Pratt, of Brighton, a careful observer, sent me a drawing of the floor of "Plato," a walled plain in the northern part of the moon, as seen with his 8 $\frac{1}{4}$ -inch silvered-glass reflector: the drawing contained eleven spots; previously no more than five or six were known. The positions of these spots differed so greatly from those with which I was acquainted, that I was induced to request that observations of Plato might be sent to me; and I have received between March 1869 and December 1870 as many as 1594, contributed by the following gentlemen:—Mr. Crossley of Halifax, Mr. Gledhill of Mr. Crossley's observatory, Mr. Pratt of Brighton, Mr. Elger of Bedford, Mr. Birmingham of Tuam, Mr. Joynson of Liverpool, Mr. Cook of Preston, Mr. Whitley of Truro, Mr. Ormesher of Patricroft, and Messrs. Ingall and Neison of London.

Having ascertained the positions of the spots observed previously to 1869, one of the first steps was to identify them; and in the course of that year every one was reobserved and several new ones added (see Plate III., which contains a plan of Plato with the estimated positions of the spots). As the observations

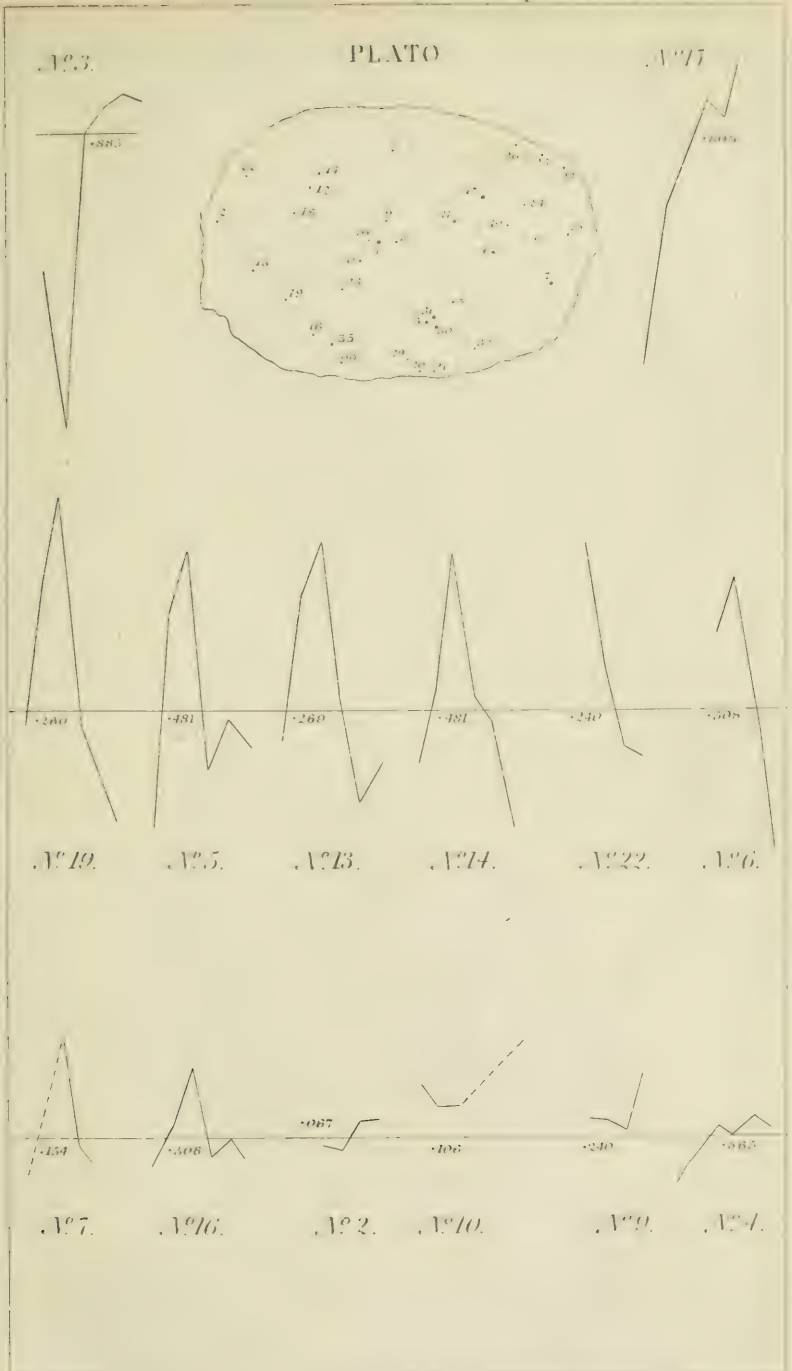
* Communicated by the Author.

accumulated I found myself in a position to attempt a discussion of them, and adopted the following method:—A ledger containing as many columns as spots having been prepared, each observation of each spot was posted to its appropriate column, and a series of numbers obtained which represented the “visibility” of each: for example, the spot No. 1, nearly centrally situated, was seen more frequently than any of the others; the number of observations, therefore, would be a measure of its comparative high visibility. Taking this spot as the standard, the ratios between the observations recorded of it and those of the other spots respectively would be the degrees of visibility appertaining to each. The following Table contains the degrees of visibility of each spot as determined from the observations of twenty lunations:—

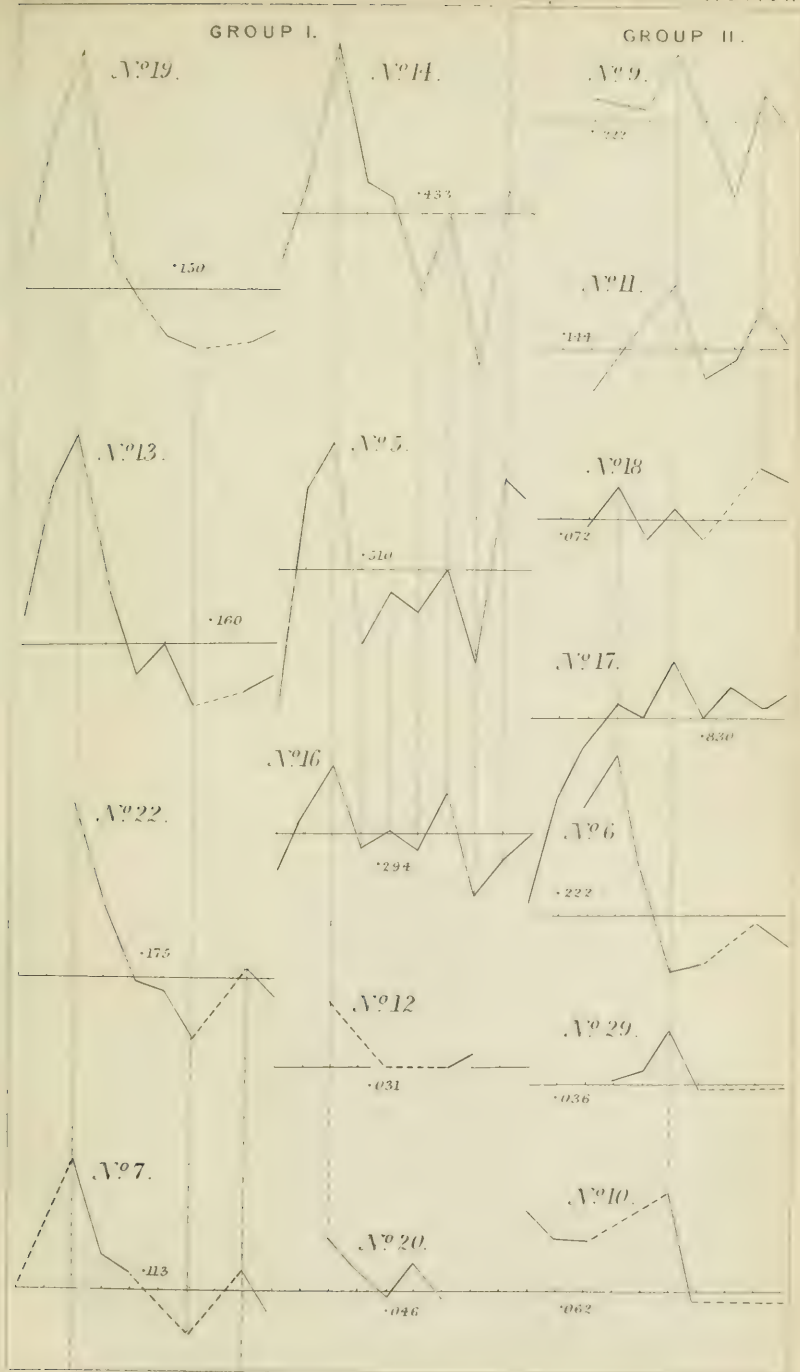
No.	Obs.	Vis.	No.	Obs.	Vis.	No.	Obs.	Vis.
0.	10	·044	13.	34	·148	25.	33	·144
1.	229	1·000	14.	99	·432	26.	1	·004
2.	9	·039	15.	4	·017	27.	2	·009
3.	207	·904	16.	67	·293	28.	1	·004
4.	204	·891	17.	192	·838	29.	8	·035
5.	121	·528	18.	19	·083	30.	38	·166
6.	49	·214	19.	31	·135	31.	6	·026
7.	24	·105	20.	9	·039	32.	16	·070
8.	3	·013	21.	5	·022	33.	3	·013
9.	50	·218	22.	39	·170	34.	5	·022
10.	13	·057	23.	11	·048	35.	1	·004
11.	33	·144	24.	11	·048	36.	1	·004
12.	6	·026						

The agencies known to affect the appearances of lunar objects are illumination, reflection, and libration, which are more or less *regular* in their operation; consequently the variations in visibility dependent upon them should also be regular, and exhibit phenomena of periodicity. Another and much more powerful agency in affecting the visibility of such objects as we are now dealing with consists in the variable conditions of our own atmosphere, which are far from being regular in their occurrence. Before we can arrive at a sound conclusion on the phenomena presented by the spots, it is necessary to eliminate the effects of these agencies; and no better mode presents itself than that of obtaining a long series of observations, during the period of which opposite effects are likely to be compensated and differences arising from instruments, observers, &c. neutralized.

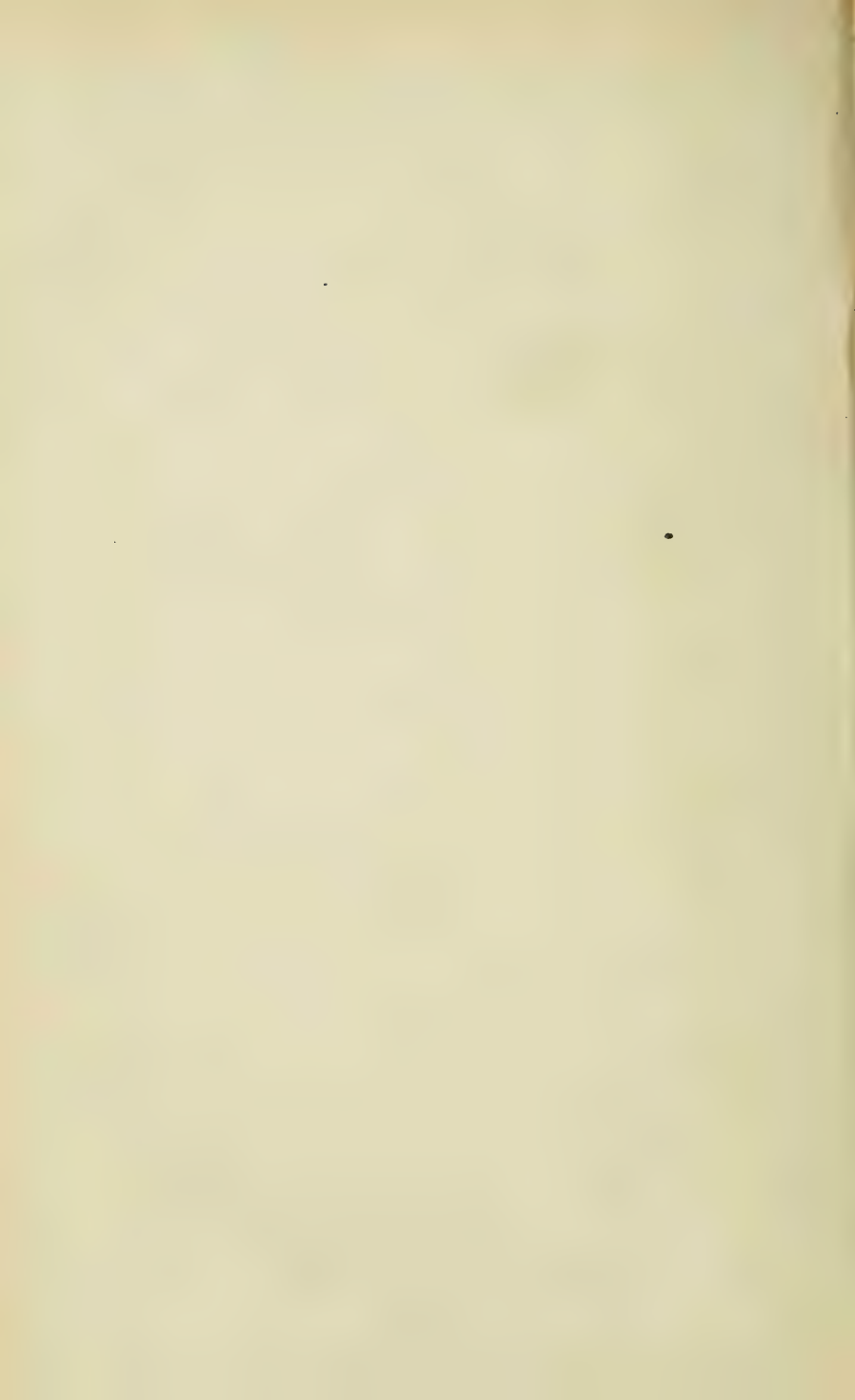
In order to ascertain, if possible, the agencies affecting the spots, the degrees of visibility have been computed for every pair of lunations from April 1869 to November 1870 inclusive, and the results projected in curves. Upon the supposition of no other agencies than those above named affecting the spots, we



Curves of visibility 12 Lunations April 1869 to March 1870.



Curves of visibility 20 Lunations April 1862, to November 1870.

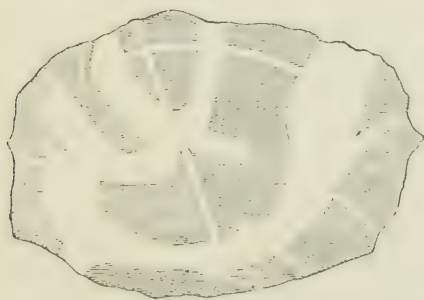


ought to have, after a sufficient interval for determining normal visibilities, a regularity of the phenomena observed; for example, the curves ought to present *regular* maxima and minima corresponding to changes of illumination, reflection, and visual ray, if these agencies are capable of affecting visibility. And as regards the earth's atmosphere, which does affect in no small degree "visibility," the whole of the spots, we might suppose, would present similar inflexions of their curves; indeed the area of Plato is so small in comparison with that of the disk, that it is difficult to conceive how any of the spots can be differently affected by changes in our own atmosphere, further than that those within reach of vision under all circumstances of change in our atmosphere would exhibit at all times nearly the same degrees of visibility, while small and faint spots (seen only in the finest weather and with the largest apertures) would, as the observations proceed, continually decline in visibility.

In the first trial, purely tentative as it was, the curves of spots Nos. 19, 5, 13, 14, 22, 7, and 16 were compared, and it was found that they presented well-marked acuminate maxima in August and September 1869 (see Plate III.). These, with the exception of No. 7, are situated on the western part of the floor.

In addition to the spots on the floor of Plato, there are certain markings, as represented in the accompanying sketch (fig. 1).

Fig. 1.



These markings, which are very variable in visibility and intensity, are given of a uniform whiteness. There is reason to believe that some have disappeared; indeed it is not expected that on any occasion the floor can be seen as represented in the sketch; it is only intended to indicate those portions which during the last fifteen years have been free from markings; those on the sketch have all been seen since the beginning of 1860, and by far the greatest number within the last two years. Four light streaks crossing the floor from north to south are given in the first edition of Beer and Mädler's large map, and mentioned in *Der*

Mond, p. 267; they are scarcely, if at all, perceptible in the new edition of the map. Space and time are important elements in unravelling the complexity of natural phenomena; and in the more extensive basis furnished by the observations of twenty lunations, it is essential to combine the two in order to arrive at a just appreciation of the results. Plate IV. contains the more important curves obtained in the course of the twenty lunations; and while they necessarily present the features included in the curves of Plate III., the additional portions furnish evidence of the operation of agencies confined apparently to certain localities, and which accordingly cannot be referred to such as are extraneous to the moon; for example, illumination, reflection, &c.

On Plate IV. may be found several pairs of similar curves, the most striking pair being that of Nos. 19 and 13. These spots, with No. 16, are situated on a somewhat bright curved streak on the north-west part of the floor. The peculiarities of the curves of these spots consist of the bold maxima of August and September 1869, which signify that in those lunations the spots were very frequently seen, much more so than either previously or since. Another feature is the rarity of visibility of the spots Nos. 19 and 13 during the last six lunations. The curve of the neighbouring spot No. 16 is not characterized by this rarity of visibility; on the contrary, it actually increased, and the spot was seen more frequently at the time when the minima of Nos. 19 and 13 occurred. The spot whose curve most nearly agrees with that of No. 16 is No. 5. It will be seen from Plate IV. that, with only one exception (October and November 1870), the inflexions of the curves are similar, but the range in the case of No. 16 is less. The logical inference is that the variations of visibility of the two spots were produced by the same agency, or at all events by similar agencies.

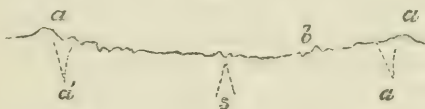
The most remarkable circumstance connected with the curve of No. 16 is its departure from the type of its neighbour No. 19, and its conformity to the curve of No. 5. It is difficult to understand this unless we take into consideration the structure of Plato. Crossing the plain from N.W. to S.E. is a fault which has dislocated the border in two opposite points, each marked by a gap or depression in the wall. Now the spots Nos. 5 and 16 are not far removed from this fault on the west; and if we were to admit a local action connected with this fault, we might probably obtain an explanation of the coincidence of the forms of the two curves furnished by spots so widely separated as Nos. 16 and 5.

A third pair of similar curves are those of Nos. 22 and 7, still more widely separated than Nos. 16 and 5, No. 7 being near the east border, and No. 22 near the west border. The spots are

nearly opposite the one to the other; and the inflexions of the curves, which in the early portions resemble those of Nos. 19 and 13, are nearly similar. This similarity is not, as in the case of Nos. 16 and 5, explicable upon the presence of a fault; nevertheless the four spots agree in being situated near the border. Now it has been ascertained by careful observation under oblique illumination, that the floor of Plato *dips* towards the border as if a fissure existed just within it. The similarity of the two sets of curves, the spots being near the border, is remarkable, and goes far to connect in some way the phenomena of visibility with the localities of the spots.

While upon this part of the subject, it may be well to solicit attention to the remarks of Scrope and Hopkins relative to the formation on the earth of tracts similar to Plato on the moon. If with Scrope we suppose the expansion of subterranean beds of crystalline or other rock to have taken place at a great depth, under *aa*, fig. 2, elevating the overlying strata, and, with Hopkins, that when the surfaces thus elevated attained a condition

Fig. 2.



at which the tension and cohesion just balanced each other, the slightest increase of tension would rupture the surface and produce two systems of fissures, which might be considerably augmented by earthquake-waves accompanied by the sudden subsidence of the tracts between the two principal lines of fissures,—we have the conditions for the production of such a region as Plato, the annular fissure opening outwardly being indicated by the dip of the floor to the border. Within and below the area, however, another fissure must occur, as at *s*, opening in the reverse direction, *i. e.* towards the lava which may exist in the interior, and which would ascend in the fissure and perhaps force its way through some minor cleft to the surface. Plato presents all the features which are characteristic of its having had such an origin: the floor, some 3800 feet below the general summit of the wall, may be regarded as evidence of its having at some anterior period *subsided* not a very great depth compared with the diameter (60 miles); the general smoothness of the floor may have resulted from the upwelling of fluid material through the central and other orifices; the existence of the “fault” across the floor may testify to its having been shaken and dislocated by an “earthquake,” the openings in the wall

strengthening the evidence; and the spots, intermittent in their visibility but persistent in their existence, may probably be referred to those minor openings found more or less in the neighbourhood of fissures.

The curve of spot No. 14, situated nearly midway between spots Nos. 5 and 22, is the most remarkable of the series, partaking, on the one hand, of the features of the curves of Nos. 19, 13, and 22 in the increase of visibility in August and September 1869, and, on the other, of Nos. 5 and 16 in the inflections of the curves during the six lunations February to July inclusive; it also agrees with the curve of No. 5 in the maximum of August and September 1870. Of all the spots, No. 14 has manifested the greatest variation of visibility.

Not only are the spots which manifested increased visibility, as stated above, restricted in "locality," but the epoch August and September 1869, at which this increase occurred, is very marked, and decidedly separates the nine spots Nos. 19, 13, 22, 7, 14, 5, 16, 12, and 20 from all the others, constituting them a distinct group, as given in Plate IV. It is to be borne in mind that this group is about a *fourth* part of the whole number of spots yet known on Plato, and the values of the maximum degrees of visibility so great as to preclude the idea that a favourable state of the earth's atmosphere is the *only* element which contributed to their development.

A second manifestation of increased visibility occurred in February and March 1870, which, like the former, was restricted in locality. This, however, was not the same, but extended as a band south of spots Nos. 1 and 4; and two only at a considerable distance from this band, viz. Nos. 13 and 20 of the first group, were affected, and that in a very subdued degree. The additional spots forming the second group are Nos. 18, 9, 11, 17, 10, and 29. The forms of the curves of these spots, given on Plate IV. group II., are suggestive of the spots themselves having been subjected either to very different influences from those which affected the spots of the first group; or, if the *same* agencies were in operation on both occasions, it is very certain that such agencies had a very restricted range indeed, and were confined to a very small portion of the moon's surface, so small that the effects of changes in the earth's atmosphere must be manifested over a much greater extent of the moon's surface.

Maxima of visibility of a somewhat subdued character, except in the case of spot No. 5, occurred in August and September 1870 over a somewhat larger area than that embraced by group II. On this occasion neither of the spots Nos. 13 and 20 were affected, but Nos. 22 and 7 exhibited increased visibility. Reference has already been made to the similarity of the curves of

these spots. Nos. 5 and 14 are also spots which were not affected at the epoch of February and March 1870; and these with No. 22 form, as it were, an outlying area to the locality of group II. Spots Nos. 6 and 7 also form an additional area on the east, so that the increase of visibility of the spots of group III., viz. Nos. 22, 14, 5, 18, 9, 11, 6, and 7, occurred over an area extending from the west to the east border of Plato. It is remarkable that spots Nos. 17 and 10 did not increase in visibility at this epoch, although situated on the area of group II.; nor did No. 29; but this spot is situated near the north border.

In connexion with the third group, and especially with spot No. 5, is a phenomenon of which mention should be made. In the spring of 1870, some months after the maximum visibility of No. 5 in August and September 1869, a streak which had not before been recorded was seen extending from No. 5 towards No. 14 as a faint object. In December 1870 a streak was observed on the opposite side or eastward of No. 5; the two form an easy and at present somewhat bright object connecting two arms of the "trident."

From a careful consideration of the whole of the phenomena presented during the twenty lunations, especially the three groups of maxima at the epochs above stated, I am strongly disposed to regard the agencies at present known capable of affecting the appearances and visibility of lunar objects as perfectly *inadequate* to produce the phenomena indicated by the curves. The true nature of the agencies that have been instrumental in their production can only be arrived at by long-continued observation. With a double length of curve, resulting from the observations of twenty additional lunations, we shall be better able to determine if the curves of such neighbouring spots as Nos. 19, 13, and 22 will *retain* the features which are now so characteristic of them—or that the inflections of the curves of Nos. 5 and 16 and of 22 and 7 will be continued, indicative of a physical connexion of some kind between spots so widely separated as they are. In the mean time, as the observations proceed these particular features may be confirmed, or new relations developed, leading us onwards to a clearer view and a juster appreciation of those forces which it is not impossible may yet be in operation on the moon's surface.

XXVI. *On the Influence of Density and Temperature on the Spectra of Incandescent Gases.* By F. ZÖLLNER*.

1. **E**VERY gaseous body can, with respect to the rays emitted from it, exist in two states essentially different from each other. In the one the rays can be spread out into a so-called *discontinuous* spectrum, with conspicuous maxima of brightness; in the other, into a *continuous* spectrum.

Both states are, analogously to the states of aggregation, merely functions of the pressure and temperature. The researches of Frankland, Wüllner, and others have proved that, in general, the *discontinuous-spectrum* state passes into that of the *continuous spectrum* by rise of temperature. But the transparency of the incandescent body is common to both states; and careful consideration of this property, in connexion with those of the function denoted by J in Kirchhoff's treatise "On the Ratio between the Emissive and the Absorptive Power of Bodies for Heat and Light"†, is, I believe, sufficient to explain the following phenomena:—

(1) The widening of the lines of discontinuous gas-spectra by increase of pressure.

(2) The change of a discontinuous spectrum into a continuous one by increase of pressure.

(3) The continuousness of the spectra of incandescent bodies in the solid or liquid state of aggregation.

(4) The dependence of the ratio between the intensities of two lines of the spectrum on pressure.

(5) The dependence of the different orders of spectra on temperature.

2. Let E_λ denote a homogeneous quantity of light, of the wave-length λ , emitted perpendicularly from the unit of surface of an infinitely large plane, luminous layer of gas of the unit of thickness; and let A_λ denote the quantity of light of the same wave-length absorbed by this layer, in terms of the quantity of the incident light. If we imagine a series of any number m of such layers, and calculate the quantity of light $E_{\lambda m}$ emitted perpendicularly from a unit of surface of the layer of the thickness m thus formed, we shall obtain for it the following expression:—

$$E_{\lambda m} = \frac{1 - (1 - A_\lambda)^m}{A_\lambda} \cdot E_\lambda. \quad . \quad . \quad . \quad (1)$$

Let E_{λ_1} and A_{λ_1} denote the corresponding magnitudes for a wave-length λ_1 very little different from the above, so that, on

* Translated from a separate impression, communicated by the Author, from the *Berichte der Kön. Sächs. Gesellschaft der Wissenschaften, math.-phys. Classe*, Oct. 31, 1870, p. 233.

† Poggendorff's *Annalen*, vol. cix. p. 291 et seqq.

the formation of a spectrum of the light emitted from the gas layer considered, the values λ and λ_1 correspond to two *immediately adjacent* parts of the spectrum; we shall have

$$E_{\lambda,m} = \frac{1 - (1 - A_{\lambda_1})^m}{A_{\lambda_1}} \cdot E_{\lambda_1}.$$

It needs not particular mention that in the concrete case, in which the breadth of the spectrum-parts compared may be not infinitely small, the values of λ and λ_1 are to be taken as mean values for the adjacent narrow bright bands of the spectrum. For the ratio of brightness of these adjacent parts we obtain accordingly, putting $\frac{E_{\lambda}}{A_{\lambda}} = J_{\lambda}$ and $\frac{E_{\lambda_1}}{A_{\lambda_1}} = J_{\lambda_1}$:—

$$\frac{E_{\lambda,m}}{E_{\lambda,m}} = \frac{[1 - (1 - A_{\lambda_1})^m] J_{\lambda}}{[1 - (1 - A_{\lambda_1})^m] J_{\lambda_1}}. \quad . \quad . \quad . \quad (2)$$

3. The magnitudes J_{λ} and J_{λ_1} are two values of the ratio of the emissive to the absorptive power for two very slightly different values of the wave-length at the same temperature. But this magnitude, multiplied by a constant, is nothing else but the function of the wave-length and temperature denoted by J in Kirchhoff's above-mentioned treatise.

This function has the following remarkable properties :—

(1) J is independent of the peculiar properties of the body (*cf.* Kirchhoff, *l. c.* p. 392).

(2) At constant temperature, therefore, in one and the same spectrum, J varies continuously with the wave-length till the value of the latter is reached at which J vanishes (*ibid.* p. 393).

(3) It may be pronounced in the highest degree probable that the function J , at constant temperature, presents no conspicuous maxima or minima with the change of wave-length (*ibid.* p. 393).

From the last two properties of the function J , it follows that “when, in the spectrum of an incandescent body, strongly pronounced maxima or minima are seen, its absorptive power, considered as a function of the wave-length of the incident rays, must have conspicuous maxima or minima with those values of the wave-length.”

By the help of this principle, Kirchhoff infers theoretically the possibility of reversing the discontinuous spectra of flames; and since this inference is confirmed by observation, conversely an empiric verification of the two properties mentioned of the function J may herein be seen.

Hence, in the present case, the two values $\frac{E_{\lambda}}{A_{\lambda}}$ and $\frac{E_{\lambda_1}}{A_{\lambda_1}}$, for the

only slightly different values λ and λ_p , can likewise be but little different from each other.

Now, as in the above expression (2) the magnitudes A_λ and A_{λ_p} , by their definition, can only be positive and never greater than unity, and therefore $1 - A_\lambda$ and $1 - A_{\lambda_p}$, must always be proper fractions, that expression is convergent for increasing values of m or of A to a limiting value which is reached when $m = \infty$ or when $A_\lambda = A_{\lambda_p} = 1$. In both these cases we have simply

$$\frac{E_{\lambda m}}{E_{\lambda, m}} = \frac{J_\lambda}{J_{\lambda_p}}.$$

This may be verbally expressed as follows:—*The ratio of brightness of two adjacent parts of a discontinuous spectrum constantly diminishes with the multiplication of the luminous layers, or as the coefficient of absorption of the same layer becomes greater, to that value which, for the same wave-length and the same temperature, corresponds to the continuous spectrum of a body which at the given thickness is perfectly opaque and black.*

This diminution of the ratio of brightness of two immediately adjacent parts of the spectrum with simultaneous increase of their brightness must necessarily make itself perceptible to the eye, first, as a widening of the lines by diminishing sharpness of their borders, and then gradually as incipient continuity of the entire spectrum.

4. It can now be shown that, *cæteris paribus*, increase of the density of a luminous gas must produce precisely the same effect as the above-considered multiplication of the layers; for if we regard the weakening produced by absorption upon a ray as the effect of a definite sum of particles which the ray meets with in its passage through the absorbing medium, it follows that the magnitude of the absorption thus produced is dependent only on the number and not on the distribution of the operative particles, provided we may assume that their absorptive effect is independent of their distance*. This assumption becomes the more probable the

* In order to prove by experiment the admissibility of assuming this in a given case, (for example, the solution of a colouring substance in various quantities of the solvent), only homogeneous light, obtained by using the narrow bands of a spectrum, can be employed in the requisite photometric measurements. The employment of coloured glass must *a priori* be regarded as inadmissible for this purpose, since, strictly speaking, it always transmits all the rays, only in different degrees of intensity, so that when the quantity of light is sufficiently small, or the glass thick enough, the rays most weakened are almost or quite imperceptible to the eye.

If J_λ denotes the intensity belonging to the wave-length λ in a given spectrum, A_λ the coefficient of absorption of a coloured medium for the same wave-length and the unit of density, the quantity of light emitted

more widely the individual particles of the absorbent medium are separated—that is, the less its density is. Accordingly, if the density of such a medium is altered, the ray passing through it has a different number of absorbent particles to pass in an equal length of its path; and it is easily seen that, on the assumption made, the number must change proportionally to the density.

If, then, the values of A_λ and $A_{\lambda'}$ in the above expressions are simultaneously referred to the unit of density, the quantity of light *transmitted* by the same layer of density σ is expressed, in parts of the incident light, by $(1 - A_\lambda)^\sigma$ and $(1 - A_{\lambda'})^\sigma$; consequently the quantity of light absorbed is expressed by:—

$$1 - (1 - A_\lambda)^\sigma = A_{\lambda\sigma},$$

$$1 - (1 - A_{\lambda'})^\sigma = A_{\lambda'\sigma}.$$

Analogously to the absorption let E_λ and $E_{\lambda'}$ be referred to the quantity of light emitted at the unit of density; and accordingly let $E_{\lambda\sigma}$ and $E_{\lambda'\sigma}$ denote the quantity of light of that layer corresponding to the density σ . As the temperature and wave-length are supposed to be constant during these alterations, from the above-mentioned properties of Kirchhoff's function the following

from the whole spectrum can be represented as the value of the following integral,

$$\int d\lambda J_\lambda (1 - A_\lambda),$$

in which J_λ and A_λ are functions of λ , and the integral is to be extended to all the values corresponding to the spectrum.

With another density σ of the absorbent medium, on the assumption of the law of absorption, this integral is transformed into the following,

$$\int d\lambda J_\lambda (1 - A_\lambda)^\sigma.$$

For a definite value of σ the value of a mean coefficient of absorption B can now indeed always be found, by virtue of which we have the following equation,

$$\int d\lambda J_\lambda (1 - A_\lambda)^\sigma = (1 - B)^\sigma \int d\lambda J_\lambda;$$

but it is evident that this equality cannot exist for other values of σ with the value of B constant. On this account it is inadmissible to calculate, by means of such a mean coefficient of absorption, and applying the above law of absorption, the quantities of light transmitted through a coloured liquid in different degrees of concentration, and to found conclusions on the difference between calculation and observation. *Such conclusions would only be valuable if the change of thickness of the layer passed through by the rays, with constant concentration, produced a different effect from that produced by the concentration being altered in the same ratio, the thickness of the layer remaining constant.* In this kind of experiments it would be simplest to decide by observations *alone* whether the absorbent effect of the individual elements of an absorbent medium is dependent or not on their distance (compare Pogg. Ann. vol. cxli. p. 69 et seq.).

equations must ensue:—

$$\frac{E_{\lambda\sigma}}{A_{\lambda\sigma}} = \frac{E_{\lambda}}{A_{\lambda}},$$

$$\frac{E_{\lambda_i\sigma}}{A_{\lambda_i\sigma}} = \frac{E_{\lambda_i}}{A_{\lambda_i}}.$$

Hence, if we introduce the above values for $A_{\lambda\sigma}$ and $A_{\lambda_i\sigma}$ and again put

$$\frac{E_{\lambda}}{A_{\lambda}} = J_{\lambda},$$

$$\frac{E_{\lambda_i}}{A_{\lambda_i}} = J_{\lambda_i},$$

we obtain for the ratio of brightness of two adjacent parts of the spectrum, as a function of the density of the incandescent gas, the following expression:—

$$\frac{E_{\lambda\sigma}}{E_{\lambda_i\sigma}} = \frac{[1 - (1 - A_{\lambda})^{\sigma}] J_{\lambda}}{[1 - (1 - A_{\lambda_i})^{\sigma}] J_{\lambda_i}}. \quad . \quad . \quad . \quad (3)$$

Since at constant temperature the value of σ alters proportionally to the pressure, this formula shows that with increasing pressure a widening of the lines of the spectrum must take place, which gradually passes into continuity of the entire spectrum.

Further, it must here be remarked that these phenomena are, within certain limits, independent of the particular nature of the function according to which the coefficient of absorption of a substance changes with its density, supposing only that this coefficient continuously increases with the density and converges towards the value 1. Greater than 1 its value cannot become without contradicting its definition. On this account, also, the coefficient of absorption of a substance cannot continually increase proportionally with the density, because otherwise there would be a value of the latter at which the case mentioned would occur.

If, now, we consider that with bodies in the liquid or solid state the coefficient of absorption has extraordinarily far greater values than with gaseous bodies, it is explained why the spectra of the denser bodies must in general be continuous.

When the compared parts of the spectrum are not adjacent, but λ and λ_i belong to sufficiently distant spectrum-lines, the preceding formula shows that this ratio also is a function of the pressure, which with continuous increase of the latter continuously approaches the limiting value $\frac{J_{\lambda}}{J_{\lambda_i}}$.

5. In order to illustrate, in an example, by numerical values

the results obtained, definite values for the magnitudes A_λ , $A_{\lambda'}$, $\frac{J_\lambda}{J_{\lambda'}}$ may be inserted in the formula (3),

$$\frac{E_{\lambda\sigma}}{E_{\lambda'\sigma}} = \frac{[1 - (1 - A_\lambda)^\sigma] J_\lambda}{[1 - (1 - A_{\lambda'})^\sigma] J_{\lambda'}},$$

and then the magnitudes $\frac{E_\lambda}{E_{\lambda'}}$ may be calculated for different values of σ .

Let $A_\lambda = 0.100$,

$$A_{\lambda'} = 0.005.$$

For immediately adjacent parts of the spectrum

$$\frac{J_\lambda}{J_{\lambda'}} = 1.$$

For distant parts (for example, for the hydrogen-lines C and F), let

$$\frac{J_\lambda}{J_{\lambda'}} = 0.25,$$

and, for simplicity, let the above values of A_λ and $A_{\lambda'}$ be retained. We thus find the following series for the ratio of brightness with ascending values of σ :—

of adjacent parts, $\frac{J_\lambda}{J_{\lambda'}} = 1.$		Ratio of brightness of a spectrum with different pressures.		of distant parts, $\frac{J_\lambda}{J_{\lambda'}} = 0.25.$	
$\sigma.$	$\frac{E_{\lambda\sigma}}{E_{\lambda'\sigma}}.$		$\sigma.$		$\frac{E_{\lambda\sigma}}{E_{\lambda'\sigma}}.$
1	20.0		1		5.00
10	13.3		10		3.33
20	9.2		20		2.30
30	6.8		30		1.70
40	5.3		40		1.33
50	4.5		50		1.13
60	3.8		60		0.95
70	3.4		70		0.85
80	3.0		80		0.75
90	2.7		90		0.67
100	2.5		100		0.63
200	1.6		200		0.40
300	1.3		300		0.32
400	1.2		400		0.30
500	1.1		500		0.28

In the case first considered, for the value $\sigma=1$, the spectrum-line investigated would appear 20 times as bright as its immediate environs, and hence might, to the eye, stand out as a sharp bright line from a dark ground. If, for example, the pressure of 1 millim. of mercury in a Geissler's tube corresponds to the value $\sigma=1$, on the pressure being increased to 500 millims. the brightness of the parts of the ground immediately adjacent to the line would be to that of the line in the ratio of 10 to 11; so that the latter would be already much widened, and its edges must appear indistinct.

The second case shows that, by compression of the incandescent gas, different parts of the spectrum may reverse their ratio of brightness. In the example cited, at the commencement of the compression the brightness $E_{\lambda\sigma}$ of a line belonging to the wave-length λ is five times that ($E_{\lambda_1\sigma}$) of one the wave-length of which is λ_1 . With 50 times this pressure the two lines have nearly equal brightness. But if the pressure is increased to 500 times its original amount, the line which at first was the darker becomes nearly 4 times as bright as the other.

If the density diminishes constantly to 0, the ratio of brightness approaches the value $\frac{0}{0}$. Differentiating according to σ the numerator and denominator of the expression for $\frac{E_{\lambda\sigma}}{E_{\lambda_1\sigma}}$ and supposing $\sigma=0$, we find for that limiting value

$$\frac{J_{\lambda} \log (1 - A_{\lambda})}{J_{\lambda_1} \log (1 - A_{\lambda_1})}.$$

Referring J_{λ} and J_{λ_1} to adjacent values of λ , so that $\frac{J_{\lambda}}{J_{\lambda_1}}$ may be supposed $=1$, we find that with continued diminution of σ the contrast with which a bright line stands out from the ground approaches asymptotically a maximum value which is expressed by

$$\frac{\log_n (1 - A_{\lambda})}{\log_n (1 - A_{\lambda_1})}.$$

For the above adopted values of A_{λ} and A_{λ_1} , this value would be 20.8, and hence would but little differ from that taken by the ratio of brightness for $\sigma=1$.

6. Our considerations hitherto have extended to the changes of intensity in two different parts of the spectrum; but the developed formulæ account for the alterations of intensity which take place in one and the same place in the spectrum, at constant temperature, by alterations of the pressure.

According to what precedes, the expression for the brightness

belonging to a definite wave-length λ , with the density σ of the incandescent gas, is the following:—

$$E_{\lambda\sigma} = [1 - (1 - A_{\lambda})^{\sigma}] \frac{E_{\lambda}}{A_{\lambda}}.$$

Accordingly $E_{\lambda\sigma}$ vanishes for $\sigma=0$, and for $\sigma=\infty$ reaches the maximum value $\frac{E_{\lambda}}{A_{\lambda}}$.

For a given value of σ and a given temperature, $E_{\lambda\sigma}$ for a certain value of λ will be an absolute maximum; or, in other words, among the various bright lines of a discontinuous spectrum, *one* will be the brightest, *since in a given spectrum both* A_{λ} *and* $\frac{E_{\lambda}}{A_{\lambda}}$ *vary as functions of* λ .

Having regard, now, to the fact that, so soon as the value of $E_{\lambda\sigma}$ sinks below a certain limit (given by that of the sensibility of our eye), the place in question of the spectrum vanishes to our perception, from these considerations results the following theorem:—

If with the temperature constant the density of an incandescent gas is constantly diminished, the number of the lines of its spectrum must also be diminished and finally, in general, the whole spectrum be reduced to a single line, the situation of which depends on the temperature and quality of the gas.

I believe that this theorem may be regarded as confirmed by the observations published during the past year, by Frankland and Lockyer, in the Proceedings of the Royal Society, No. 112. The passage in question is as follows:—

“Under certain conditions of temperature and pressure, the very complicated spectrum of hydrogen is reduced in our instrument to one line in the green corresponding to F in the solar spectrum.

“The equally complicated spectrum of nitrogen is similarly reducible to one bright line in the green, with traces of other, more refrangible, faint lines.”

Yet these observations do not permit us at once to draw conclusions as to the temperature of those heavenly bodies which, like many of the nebulae, present the remarkable phenomenon of very simple spectra; the preceding considerations show that such conclusions would be inadmissible, *since, at any temperature, sufficient rarefaction of the incandescent gas may reduce its spectrum to a single line, the situation of which, in the case of one and the same substance, is dependent on the temperature only.*

‡ Having regard to the above-demonstrated principle of the equivalence of the density and the thickness of the radiating layer, it may even be maintained that the values of the temperature

and density with which a gas in a Geissler's tube shows such simple spectra may agree not remotely with those of the nebulae, because in the latter the enormous thickness of the radiating layer supposes an almost infinite rarefaction of the luminous gas.

On the other hand, it is obvious that the continuity of a nebular spectrum is not sufficient to permit a conclusion as to the density, because, according to the above-mentioned principle, the same effect may also be produced by a sufficient thickness of the radiating layer.

Meanwhile the following consideration of the subject may at least serve to determine the lower limit of the temperature of a nebula the spectrum of which is discontinuous:—

The expression for the brightness $E_{\lambda\sigma}$ of the place belonging to the wave-length λ in the spectrum of a gas with the density σ , and at a given temperature, is

$$E_{\lambda\sigma} = [1 - (1 - A_{\lambda})^{\sigma}] \frac{E_{\lambda}}{A_{\lambda}}.$$

As already remarked, this expression cannot become greater than $\frac{E_{\lambda}}{A_{\lambda}}$; and, according to Kirchhoff's theorem, this value is that brightness which, for equal temperature and wave-length, is possessed by the same place in the spectrum of a perfectly black body, and, in fact, *independent of its other qualities**. Hence, if, with the current from a galvanic battery, we heat a dark, opaque body (one as nearly as possible corresponding to the requirements stated—for example, a piece of charcoal), and produce a spectrum from the light emitted, the temperature of the incandescent charcoal will be lower than that of the luminous gas with a *discontinuous* spectrum, as long as the brightness of the continuous spectrum, in the place corresponding to a bright line of the gas-spectrum, is *less than or equal to* the brightness of this line. It is here assumed that, *ceteris paribus*, $\frac{E_{\lambda}}{A_{\lambda}}$ continually increases with the temperature.

If we now compare the brightness of a line in the nebular spec-

* It is easily seen that the above expression, when $\sigma = \infty$, expresses the perfect opacity of the layer of gas, since $1 - (1 - A_{\lambda})^{\sigma}$ expresses, in terms of the incident light of the wave-length λ , the quantity of light absorbed by this layer, $A_{\lambda}\sigma$. If, then, $A_{\lambda}\sigma = 1$, this signifies the complete absorption of a ray incident upon the mass of gas. Remembering that all bodies, even those relatively opaque, are transparent in sufficiently thin lamellae, and that, by virtue of the equivalence of thickness and density, the number m of the radiating and absorbing layers may be put in the above formula instead of the density σ , we perceive the applicability of the above expression to opaque bodies also, since it involves at the same time the necessity of the continuity of their spectra.

trum with the brightness of the homologous place in the charcoal-spectrum, we can, neglecting the absorption in cosmical space, and taking due account of that in our atmosphere, ascertain in the manner indicated a *lower* limit for the temperature of the nebula, as soon as we succeed in determining the temperature of the galvanically incandescent charcoal.

7. The dependence of the *position* of the lines of a discontinuous spectrum on the *temperature* and *quality* of the incandescent gas, above recognized theoretically as admissible and probable, is very remarkable, and appears to me quite adequate to explain the remarkable phenomenon, discovered by Plücker, of the so-called *spectra of different orders* of one and the same body; for the value of the expression for the ratio of brightness of two *adjacent* places in the spectrum,

$$\frac{E_{\lambda\sigma}}{E_{\lambda_1\sigma}} = \frac{[1 - (1 - A_\lambda)^\sigma] J_\lambda}{[1 - (1 - A_{\lambda_1})^\sigma] J_{\lambda_1}},$$

is dependent, when σ is constant, only on the values of the absorptive powers A_λ and A_{λ_1} , since $\frac{J_\lambda}{J_{\lambda_1}}$ for this case may always be

taken as = 1. But these values may have, for the *same wave-length* and continuous alteration of the temperature, similar maxima and minima to those which they in fact possess for the *same temperature* and continuous alteration of the wave-length, whereby they produce the phenomenon of discontinuous spectra. The simplicity and continuity attributed to Kirchhoff's function J refers only to the *ratio* between the magnitudes E_λ and A_λ , not to the magnitudes themselves. While that function is *the same* for all bodies, E_λ and A_λ (as functions of the temperature and wave-length) are directly dependent on the particular condition and character of the body. We thus see that the expression for $\frac{E_{\lambda\sigma}}{E_{\lambda_1\sigma}}$, with alteration of the temperature, may solely

through alteration of the values of A_λ and A_{λ_1} assume different values which are *greater or less than unity*. From this it follows that the *ratio of brightness of two adjacent places in the spectrum may be reversed by alterations of temperature, and a minimum appear in the place of a former maximum*.

Hence, in relation to the changes of intensity of *adjacent* parts of a spectrum, there is an essential difference between the effects of temperature and those of pressure: the ratio of intensity may be reversed by changes of temperature; its reversal is not possible by changes of pressure. By increase of the latter a difference of intensity can only be made to vanish, it cannot be reversed. When, therefore, inversions of this kind are observed in different spectra of one and the same substance, this is the result of dif-

ference of temperature *only*. The following theorem may therefore be pronounced general:—

When the difference between two spectra of one and the same incandescent gas is such that, for rays of any refrangibility, to a maximum of the one spectrum a minimum of the other corresponds, the temperatures proper to the two spectra must be different.

The origination of spectra of different orders of one and the same gas must therefore be regarded as a phenomenon evoked especially by alterations of the temperature, and not of the density of the incandescent gas.

8. In like manner as Kirchhoff empirically infers the continuity of the function J with alteration of the wave-length λ and with constant temperature, the continuity of that function with constant λ and variable temperature may also be inferred.

In reference to the first-mentioned continuity, Kirchhoff (*l. c.* p. 293) remarks as follows:—

“For a constant temperature the function J changes continuously with the wave-length, as long as the latter falls short of the value at which, for that temperature, J begins to vanish. The correctness of this assertion may be inferred from the continuity of the spectrum of an incandescent platinum wire, as soon as we assume that the absorptive power of this body is a continuous function of the wave-length of the incident rays.”

This conclusion rests upon the fact that the spectrum of incandescent black *opaque* bodies presents to our eye the totality of all the functional values of J proper to the different values of λ in juxtaposition in space as they correspond to the temperature of the incandescent body. Now, if the strength of the physiological impression produced in our sensorium by the different values of J in a spectrum were not likewise a function of the wave-length, or, in other words, could we assume that the physiologically determined ratio of intensity of two different parts of the spectrum is equal to the mechanical, the course of the function J with alteration of the wave-length could be represented simply by the photometrically determined curve of intensity of a given spectrum at constant temperature of the incandescent body.

Although, from the circumstance adduced, such a representation is not possible, and the nature of the physiological function according to which the optical impression of a homogeneous ray changes with the wave-length while the *vis viva* is constant is unknown, it may yet be maintained of this function also that it must *necessarily* be continuous*; for if this were not the case, the

* The notions “continuous” and “discontinuous” are not to be taken here in a mathematical sense, but with reference to the absence or presence of strongly prominent maxima or minima.

spectra of incandescent opaque bodies could not make a continuous impression on our sensorium, unless the discontinuity of the physiological function were such that a maximum value in the one function were always compensated by a minimum in the other. But since the assumption of such a relation between the incandescent body and our sensorium would evidently be absurd, from the continuity of the spectra of incandescent opaque bodies the continuity of the function J follows, as well as that of the physiological function of the intensity of the sensation in its dependence on the wave-length.

That the function J , for a constant value of λ , does not change discontinuously with the temperature, but much rather increases continuously therewith, may be inferred from the circumstance that, as far as observations have hitherto gone, the spectrum of an incandescent opaque body becomes *continuously* brighter in all its parts with increasing temperature, even though the quickness of this increase may be very different for different values of λ . From this it follows that, for those values of the temperature for which E possesses strongly prominent maxima or minima, A must also have such maxima or minima, and that, *in general*, the alterations undergone by E through changes of temperature must be accompanied by alterations of A *in the same direction*. Now, since, according to the observations hitherto made, the values of E for all values of λ have been found to increase with rising temperature, the values of A also must be supposed in general greater with a high than with a lower temperature.

Hence results an important consequence in relation to the conversion of a discontinuous spectrum into a continuous one, at various temperatures, by increase of the density of the incandescent gas; for if we consider the ratio of intensity of two adjacent parts of the spectrum, where for the values of λ and λ_1 (which differ but little from each other) the values of the two functions $\frac{E_\lambda}{A_\lambda}$ and $\frac{E_{\lambda_1}}{A_{\lambda_1}}$ can differ but little, and hence their ratio may be taken as equal to unity, then according to the earlier part of this memoir the expression for this ratio is

$$\frac{E_{\lambda\sigma}}{E_{\lambda_1\sigma}} = \frac{1 - (1 - A_\lambda)^\sigma}{1 - (1 - A_{\lambda_1})^\sigma}.$$

Here, the greater A_λ and A_{λ_1} , the quicker the value of $\frac{E_{\lambda\sigma}}{E_{\lambda_1\sigma}}$ converges toward unity as the value of σ increases; and the consequence of this will be, first the widening of the line in question, and finally the continuity of the whole spectrum.

In the example above adduced we put $A_\lambda = 0.100$, and $A_{\lambda_1} = 0.005$; and for these values the ratio of intensity of the

parts of the spectrum considered was diminished from 20 to 1.1 by increase of the pressure from $\sigma=1$ to $\sigma=500$. But now let these two values be supposed 5 times as great, thus,

$$A_{\lambda}=0.500,$$

$$A_{\lambda'}=0.025,$$

we then obtain the following series of ratios of intensity for different values of σ :—

σ .	$A_{\lambda}=0.500$ $A_{\lambda'}=0.025$	$A_{\lambda}=0.100$ $A_{\lambda'}=0.005$
1	20.0	20.0
10	4.5	13.3
20	2.5	9.2
40	1.6	5.3
80	1.1	3.0

Here, then, the ratio of intensity, starting from the same value, reaches the small value 1.1 at a pressure less than one eighth part of that requisite with a lower value of the absorptive power.

The results last obtained may be expressed in the two following theorems :—

The greater the absorptive power in two adjacent parts of a spectrum, the more quickly does the ratio of their intensity change with the density.

Taking into account the generally greater values of the absorptive power at higher temperature, it hence follows further that,

The higher the temperature of the incandescent gas, the more quickly is a discontinuous spectrum converted into a continuous one by increase of the density.

By the aid of the first theorem, from the great changes undergone by a certain line in a spectrum (for example, the hydrogen-line F) through alterations of the pressure, relatively great values of the absorptive power in that part of the spectrum may be inferred, provided that we duly consider the difference of the dispersion in the various parts of a spectrum produced by refraction.

From the second theorem it follows that, the density remaining the same, a discontinuous spectrum can be converted into a continuous one by a sufficient increase of the temperature of the incandescent gas. This consequence is confirmed by Wüllner's observations, in which, with equal magnitudes of the pressure, discontinuous spectra corresponded to the weaker discharges, and continuous ones (or altered in that direction) to the stronger,

the effect being the same as an increase of pressure would have produced with weaker discharges*.

9. In our investigations hitherto, only *two* parts of the spectrum have been compared. But when the object is to determine generally the changes in the contrast with which a bright line stands out from the darker ground, the alterations in the brightness of the ground *on both sides* of the line must be taken account of. Here two cases may occur: either the values of the absorptive power on the two sides are equal, so that its curve declines symmetrically from the maximum; or this symmetry does not exist. It is obvious that in the latter case (which, being evidently the most general, will be the most frequent) the widening produced in the lines by alterations of density must be quicker on that side of the line on which the absorptive power is the greater and hence its curve declines less steeply. Although, as above remarked, the *maximum* of brightness of a line cannot be displaced by alterations of density *alone*, yet the effect of the asymmetry mentioned will in general be that the middle of the line widened by increase of the pressure will no longer coincide with that of the line previously to the widening, so that, on the supposition made, the following theorem may be advanced:—

The middle of a line widened by increase of the pressure is displaced toward that side of the spectrum in which the greater values of the absorptive power of the incandescent gas are situated.

Hence we must not at once refer the displacement of the centres of unequally wide lines of the same substance in different sources of light to an alteration of the refrangibility by the approximation or removal of the luminous body. In general, such a cause must not be assumed as certainly existing until the displacement of *several* lines of the same substance, in a qualitatively and quantitatively accordant manner, has been shown.

10. The widening of lines through increase of the vapour-density of the incandescent gas can be very simply shown in the lines of sodium, if we, by pushing a grain of chloride of sodium

* Compare Pogg. *Ann.* vol. cxxxvii. p. 344 *et seqq.* Wüllner here compares the spectrum of hydrogen at a pressure of 300 millims. in a Geissler's tube, as seen when produced by discharges from a Leyden jar, with that produced at much higher pressure by discharges from the simple induction-apparatus. His words are:—

“With further increase of pressure $H\beta$ and $H\gamma$ expand continually more, so that soon they appear as only maxima of brightness on a continuously luminous background which is growing brighter; at the same time $H\alpha$ becomes gradually less sharp, indistinct at its edges, and widened, so that when the pressure of the gas is 300 millims. the spectrum produced by using the jar has about the same appearance as when, without the jar, the pressure of the gas amounted to nearly 3 atmospheres. The brightness of the spectrum is also about the same.”

more or less deeply into the flame of a Bunsen's burner, develop different quantities of sodium vapour. With small quantities the lines are fine and sharp; with large quantities, much widened, and the edges indistinct.

But in this case the more refrangible of the two lines of sodium widens considerably more than the other, so that with the greatest amount of vapour which can be developed in the given manner the more refrangible line is fully twice as broad as the other.

From this fact must be inferred, in accordance with § 8, greater values of the absorptive power for the values of λ belonging to the more refrangible sodium-line, and accordingly, on account of the continuity of Kirchhoff's function J , also greater values of the emissive power. Both these consequences are confirmed by observation.

When the two sodium-lines in the solar spectrum are compared, we find, on viewing them more carefully, the more refrangible decidedly darker than the other—a fact which proves the greater absorptive power in this part of the spectrum.

If, further, we compare the bright sodium-lines in any source of light, we always find the more refrangible brighter than the other, whether the vapour-density be great or small. This difference of brightness becomes even very striking when the following method of observation is employed. The slit of the spectroscope is gradually opened till the inner edges of the lines thus widened exactly touch. The adjacent bright bands then show such a considerable difference in brightness, that it strikes the eye at the first glance. By this observation, then, also the greater emission for the values of λ belonging to the more refrangible line D is proved.

In order, by observation of the sodium-lines, to verify the asymmetry which, according to the theory we have developed, was to be expected in the widening, with the aid of my reversion-spectroscope I brought each of the bright sodium-lines in one spectrum into coincidence with the analogous line in the other spectrum, so that each line became the prolongation of the other, and the two together apparently formed only a single line of double the length. In the more refrangible line, which with increase of the vapour-density was the most widened, no displacement was perceptible; meanwhile there appeared to take place in the other line, as the brightness increased, an extremely slight *displacement in the direction of a diminution of the refrangibility.*

Even if we attribute no force of proof to this observation, as being too uncertain, yet, on the other hand, an interesting observation made by Dr. J. J. Müller, Private Instructor and Assistant in Physics at the Physiological Institute in this place,

while engaged in other investigations, cannot well be explained otherwise than by the assumption of such an asymmetrical widening of one or the other of the two sodium-lines.

He has found that the Newton's interference-bands which, with a difference of path of about 20,000 undulations, are produced in a plano-parallel glass plate of 5 millims. thickness by homogeneous light from sodium, are displaced almost the whole distance of two rings when the intensity of the sodium-flame is varied, in the manner above indicated, by alteration of the amount of vapour. With this displacement is connected a simultaneous diminution of the sharpness of the rings as the intensity increases, until they completely vanish, this being a necessary consequence of that diminution of the homogeneity of the light which is expressed in the widening of the lines. *The magnitude and direction of this displacement points to a diminution of the refrangibility of the light, to which in the spectrum a displacement of one or the other sodium-line of at most $\frac{1}{200} - \frac{1}{100}$ of their reciprocal distance would correspond.*

Hence it is evident that this method of observation is vastly more delicate for the indication of asymmetry in the widening of the lines than the spectroscopic one, as long as its application is not prevented by the want of sufficient light.

Fuller details on these experiments will be communicated by Dr. Müller in a special memoir which will shortly be published; and I intend to treat the theorems developed in the foregoing in their application to the investigation by spectrum-analysis of the physical constitution of the heavenly bodies, and particularly to the determination of the conditions of temperature and pressure of the solar atmosphere.

XXVII. *To determine the degree of Polarization in the case of a ray of common Light falling obliquely on and being reflected or refracted by a bundle of parallel Plates.* By W. G. ADAMS, Professor of Natural Philosophy in King's College*.

REGARDING a ray of common light as equivalent to two polarized rays of equal intensity, whose planes of polarization are in and perpendicular to the plane of incidence, then a ray, of intensity 1, is equivalent to two, each equal to $\frac{1}{2}$.

The ray of intensity $\frac{1}{2}$ which is polarized in the plane of incidence will give rise to a reflected ray of intensity $\frac{1}{2}v^2$, and a refracted ray of intensity $\frac{1}{2}(1-v^2)$, where v denotes the ratio of the amplitudes of the vibrations of the ether for the reflected and the incident ray.

If ϕ and ϕ_1 be the angles of incidence and refraction on the

* Communicated by the Author.

206 Prof. W. G. Adams on the Polarization of a ray of first surface, then

$$v = \frac{\sin(\phi - \phi_1)}{\sin(\phi + \phi_1)}.$$

The ray of intensity $\frac{1}{2}$, which is polarized in a plane *perpendicular to the plane of incidence*, will give rise to a reflected ray of intensity $\frac{1}{2}v^2$, and a refracted ray of intensity $\frac{1}{2}(1-v^2)$ at the first surface, where

$$w = \frac{\tan(\phi - \phi_1)}{\tan(\phi + \phi_1)}.$$

Now consider first only the light polarized in the plane of incidence.

The ray refracted into the first plate is divided up at its second surface into an internally reflected ray of intensity $\frac{1}{2}(1-v^2)v^2$, and a refracted ray of intensity $\frac{1}{2}(1-v^2)^2$.

A similar division will take place at each surface with every successive internally reflected ray.

The intensity of the light passing out of the plate on the side on which it entered will be

$$\frac{1}{2}(1-v^2)^2 \{v^2 + v^6 + v^{10} + \&c.\},$$

and therefore the whole intensity of the reflected beam is

$$\begin{aligned} & \frac{1}{2}v^2 + \frac{1}{2}(1-v^2)^2(v^2 + v^6 + v^{10} + \&c.) \\ &= \frac{1}{2}v^2 + \frac{1}{2}(1-v^2)^2 \frac{v^2}{1-v^4} \\ &= \frac{1}{2}v^2 \left\{ 1 + \frac{1-v^2}{1+v^2} \right\} = \frac{1}{2} \cdot \frac{2v^2}{1+v^2}. \end{aligned}$$

Hence the intensity of the beam which is refracted through the plate is

$$\frac{1}{2} \left(1 - \frac{2v^2}{1+v^2} \right) = \frac{1}{2} \cdot \frac{1-v^2}{1+v^2},$$

no light being supposed to be absorbed by the plate.

Let the part which is reflected be represented by $\frac{1}{2}k^2$; then the portion which passes through will be represented by $\frac{1}{2}(1-k^2)$.

Now consider the action of the second *plate* on this portion, which has been refracted by the first *plate*. The successive reflections and refractions by the two *plates* will be of the same character as the successive reflections and refractions by the two *surfaces* of the first plate; also the intensities of the portions reflected and refracted by the two *plates* will bear the same relation to k^2 , that the intensities of the portions reflected and refracted from the two *surfaces* bear to v^2 . Therefore as the intensity of the beam refracted through the two surfaces was changed from $\frac{1}{2}$ to $\frac{1}{2} \cdot \frac{1-v^2}{1+v^2}$, so the intensity of the beam refracted through two

plates will be changed from $\frac{1}{2}$ to $\frac{1}{2} \cdot \frac{1-k^2}{1+k^2}$, i. e. to $\frac{1}{2} \cdot \frac{1-v^2}{1+3v^2}$; and the intensity of the beam reflected from two plates will be

$$\frac{1}{2} \left(1 - \frac{1-k^2}{1+k^2} \right) = \frac{1}{2} \cdot \frac{2k^2}{1+k^2} = \frac{1}{2} \cdot \frac{4v^2}{1+3v^2}.$$

Now let

$$\frac{4v^2}{1+3v^2} = k_1^2,$$

then

$$\frac{1-v^2}{1+3v^2} = 1 - k_1^2;$$

and the intensity of the beam which passes through two plates will be $\frac{1}{2}(1 - k_1^2)$.

Now consider the four plates of glass as two bundles, each consisting of two plates. Then it will be seen that the intensities of the beams reflected and refracted by the two bundles will bear the same relation to k_1^2 that the intensities of the beams reflected and refracted by the two plates bear to k^2 . Hence the intensity of the light reflected by the two bundles is

$$\frac{1}{2} \cdot \frac{2k_1^2}{1+k_1^2} = \frac{1}{2} \cdot \frac{8v^2}{1+7v^2};$$

and the intensity of the light refracted through the two bundles is

$$\frac{1}{2} \cdot \frac{1-k_1^2}{1+k_1^2} = \frac{1}{2} \cdot \frac{1-v^2}{1+7v^2}.$$

Now let us consider the ray of intensity $\frac{1}{2}$ which is polarized in a plane perpendicular to the plane of incidence. It is clear that the intensity of the beam arising at any surface from this ray will bear the same relation to w^2 that the intensity of the corresponding beam arising at the same surface from the other ray bears to v^2 ; hence the intensity of the beam from this ray,

which is reflected from four plates, $= \frac{1}{2} \cdot \frac{8w^2}{1+7w^2}$, and the intensity of the refracted beam through four plates $= \frac{1}{2} \cdot \frac{1-w^2}{1+7w^2}$.

Hence the whole intensity of the reflected beam is

$$\left(\frac{1}{2} \cdot \frac{8v^2}{1+7v^2} + \frac{1}{2} \cdot \frac{8w^2}{1+7w^2} \right),$$

and the whole intensity of the refracted beam is

$$\frac{1}{2} \cdot \frac{1-v^2}{1+7v^2} + \frac{1}{2} \cdot \frac{1-w^2}{1+7w^2}.$$

The intensity of the polarized light in each beam being

$$\left(\frac{1}{2} \cdot \frac{1-w^2}{1+7w^2} - \frac{1}{2} \cdot \frac{1-v^2}{1+7v^2}\right),$$

the intensity of natural light in the reflected beam is $\frac{8w^2}{1+7w^2}$,
and in the refracted beam is $\frac{1-v^2}{1+7v^2}$.

Hence the degree of polarization in the refracted beam is

$$\frac{\frac{1-w^2}{1+7w^2} - \frac{1-v^2}{1+7v^2}}{\frac{1-w^2}{1+7w^2} + \frac{1-v^2}{1+7v^2}} = \frac{8(v^2-w^2)}{(1+7v^2)(1-w^2) + (1+7w^2)(1-v^2)}.$$

Let $\sin(\phi - \phi_1) = y$, and $\sin(\phi + \phi_1) = x$; then $v = \frac{y}{x}$, and

$$1-w^2 = 1 - \frac{\tan^2(\phi - \phi_1)}{\tan^2(\phi + \phi_1)} = 1-v^2 \cdot \left(\frac{1-x^2}{1-y^2}\right) = \frac{1-v^2}{1-y^2};$$

also

$$v^2 - w^2 = v^2 - v^2 \left(\frac{1-x^2}{1-y^2}\right) = \frac{v^2 x^2 - v^2 y^2}{1-y^2} = \frac{y^2(1-v^2)}{1-y^2},$$

and

$$1+7w^2 = \frac{1+7v^2-8y^2}{1-y^2}.$$

Dividing out by the common fraction $\frac{1-v^2}{1-y^2}$ in numerator and denominator, and calling p and n the intensities of polarized and natural light, we get

$$\frac{p}{p+n} = \frac{8y^2}{(1+7v^2) + (1+7v^2-8y^2)} = \frac{4y^2}{1+7v^2-4y^2}.$$

From the above relations it appears that

$$1-v^2 = (1-w^2) \cdot \cos^2(\phi - \phi_1);$$

also

$$v^2 - w^2 = (1-w^2) \sin^2(\phi - \phi_1) = (1-v^2) \tan^2(\phi - \phi_1),$$

and

$$1+v^2-2y^2 = (1+w^2) \cdot \cos^2(\phi - \phi_1),$$

$$1+3v^2-4y^2 = (1+3w^2) \cdot \cos^2(\phi - \phi_1),$$

$$1+7v^2-8y^2 = (1+7w^2) \cos^2(\phi - \phi_1).$$

and generally

$$1 + (2m-1)v^2 - 2my^2 = \{1 + (2m-1)w^2\} \cdot \cos^2(\phi - \phi_1).$$

Hence

$$\frac{p}{n+2p} = \frac{4 \sin^2(\phi - \phi_1)}{1 + 7v^2} = \frac{4}{\frac{1}{\sin^2(\phi - \phi_1)} + \frac{7}{\sin^2(\phi + \phi_1)}},$$

or

$$\begin{aligned} \frac{p}{n} &= \frac{4y^2}{1 + 7v^2 - 8y^2} = \frac{4 \tan^2(\phi - \phi_1)}{1 + 7w^2} \\ &= \frac{4}{\frac{1}{\tan^2(\phi - \phi_1)} + \frac{7}{\tan^2(\phi + \phi_1)}}. \end{aligned}$$

It appears, then, for a single refraction at a single surface, that

$$\frac{\frac{1}{2}(v^2 - w^2)}{\frac{1}{2}(1 - v^2) + \frac{1}{2}(1 - w^2)} = \frac{\sin^2(\phi - \phi_1)}{1 + \cos^2(\phi - \phi_1)},$$

or the degree of polarized light in the refracted ray depends only on the deviation.

Now

$$\sin \phi = \mu \sin \phi_1;$$

and from this equation we must determine the values of

$$\sin^2(\phi - \phi_1) \text{ and } \frac{\sin^2(\phi - \phi_1)}{\sin^2(\phi + \phi_1)}$$

for different values of ϕ , from which we shall have

$$\frac{p}{p+n} = \frac{4 \sin^2(\phi - \phi_1)}{1 + 7 \frac{\sin^2(\phi - \phi_1)}{\sin^2(\phi + \phi_1)} - 4 \sin^2(\phi - \phi_1)};$$

or

$$\frac{n+p}{p} = \frac{\frac{1}{\sin^2(\phi - \phi_1)} + \frac{7}{\sin^2(\phi + \phi_1)} - 4}{4}.$$

Now suppose that before falling on the plate the light is polarized in the plane of incidence, and that p represents the part

polarized and n the natural light, then $\frac{p + \frac{n}{2}}{n + p}$ is the intensity of

the first ray, and $\frac{n}{2(n+p)}$ the intensity of the second ray; hence

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the degree of polarization in the refracted beam is

$$\begin{aligned}
 \frac{p_1}{p_1 + n_1} &= \frac{\left(p + \frac{n}{2}\right) \frac{1-v^2}{1+\tau v^2} - \frac{n}{2} \cdot \frac{1-w^2}{1+\tau w^2}}{\left(p + \frac{n}{2}\right) \frac{1-v^2}{1+\tau v^2} + \frac{n}{2} \cdot \frac{1-w^2}{1+\tau w^2}} \\
 &= \frac{p \frac{1-v^2}{1+\tau v^2} - \frac{n}{2} \cdot \frac{8(v^2-w^2)}{(1+\tau v^2)(1+\tau w^2)}}{p \frac{1-v^2}{1+\tau v^2} + \frac{n}{2} \left(\frac{1-v^2}{1+\tau w^2} + \frac{1-v^2}{1+\tau v^2} \right)} \\
 &= \frac{p(1+\tau w^2) - \frac{n}{2} \cdot 8 \tan^2(\phi - \phi_1)}{p(1+\tau w^2) + \frac{n}{2} \cdot \frac{1+\tau v^2 + 1+\tau v^2 - 8 \sin^2(\phi - \phi_1)}{\cos^2(\phi - \phi_1)}} \\
 &= \frac{p(1+\tau w^2) - \frac{n}{2} \cdot 8 \tan^2(\phi - \phi_1)}{p(1+\tau w^2) + \frac{n}{1} \cdot (1+\tau w^2) + \frac{n}{2} \cdot 8 \tan^2(\phi - \phi_1)}.
 \end{aligned}$$

Expressing the result in terms of v^2 , we get

$$\frac{p_1}{p_1 + n_1} = \frac{p(1+\tau v^2) - \left(p + \frac{n}{2}\right) 8 \sin^2(\phi - \phi_1)}{(p+n)(1+\tau v^2) - \left(p + \frac{n}{2}\right) 8 \sin^2(\phi - \phi_1)},$$

or

$$\frac{p_1}{n_1} = \frac{p - \left(p + \frac{n}{2}\right) \cdot \frac{8 \sin^2(\phi - \phi_1)}{1+\tau v^2}}{n}.$$

When the light is completely depolarized by the four plates,
then

$$\begin{aligned}
 \frac{p}{p + \frac{n}{2}} &= \frac{8 \sin^2(\phi - \phi_1)}{1+\tau v^2} = \frac{8 \sin^2(\phi - \phi_1)}{1 + \tau \frac{\sin^2(\phi - \phi_1)}{\sin^2(\phi + \phi_1)}} \\
 &= \frac{8}{\frac{1}{\sin^2(\phi - \phi_1)} + \frac{\tau}{\sin^2(\phi + \phi_1)}};
 \end{aligned}$$

hence

$$\frac{p}{n+p} = \frac{4}{\frac{1}{\sin^2(\phi - \phi_1)} + \frac{\tau}{\sin^2(\phi + \phi_1)} - 4},$$

which is the same as the value obtained for the relation of polarized to natural light when a ray of common light is refracted through the plates.

Assuming a value of $\mu=1.5$ for crown glass, we may form a Table of the degrees of polarization for different angles of incidence.

Table for $\mu=1.5$ (crown-glass). Angle of complete polarization $=56^\circ 18' 36''$, $\log \mu = .17609$, $\log 7 = .84510$.

ϕ .	10° .	15° .	20° .	$22^\circ 30'$.	25° .	30° .	35° .	40° .
$\frac{p}{n+p}$ For $\mu=1.513$.0107	.02443	.04430	.05683	.07111	.10498	.14745	.19845
$\frac{p}{n+p}$.01103	.02504	.04540	(.05813)	.07267	.1076	.1509	.2029
Probable values for $\mu=1.54$.								
$\frac{p}{n+p}$.0116	.0263	.0476076	.113	.158	.212
ϕ .	45° .	50° .	55° .	$56^\circ 18' 36''$.	60° .	65° .	70° .	72° .
$\frac{p}{n+p}$ For $\mu=1.513$.25786	.32383	.39231	.40984 (For $56^\circ 32'$)	.4560	.50530	.5309	.53305
$\frac{p}{n+p}$.2635	.3305	.3999	.4204	.4642	.5139	.5394	(.5416)
Probable values for $\mu=1.54$.								
$\frac{p}{n+p}$.275	.344	.415481	.531	.556	

From the Table, for the values of $\frac{p}{n+p}$ we see that for four plates the proportion of polarized light in the beam which passes through the plate still goes on increasing when the incidence becomes greater than the angle for complete polarization, and that at about 72° it attains its greatest value, when there is about 53 per cent. of the light polarized.

I have also formed a Table for $\mu=1.513$, of which the results are given; and from these we can derive close approximate values for the proportions of polarized light for values of μ not differing much from these values.

The probable values for $\mu=1.54$ are given in the Table.

If we employ the four plates as a depolarizer to determine the proportion of polarized light in the incident beam by reducing the light to its ordinary unpolarized state, then (as before explained) the proportion of polarized light in the incident beam will be given, for any angle of complete depolarization, by the value of

$\frac{p}{n+p}$ for that angle.

That the reasoning from the analogy between plates and refractive surfaces is correct will readily appear; and the law of intensities may at once be deduced.

Consider only the ray of intensity $\frac{1}{2}$ which is polarized in the plane of incidence. $\frac{1}{2}k^2$ is reflected by the first plate, and $\frac{1}{2}(1-k^2)$ is refracted through it. Of this latter portion, which falls on a third refracting surface, $(1-v^2)$ is refracted and v^2 is reflected; so that $\frac{1}{2}(1-k^2)(1-v^2)$ is the intensity of the refracted portion, and $\frac{1}{2}v^2(1-k^2)$ of the reflected portion. In these are included all the rays arising from the successive internal reflections *inside* the plate which take place before reflection by the third surface.

The reflected portion $\frac{1}{2}v^2(1-k^2)$ falls upon the first plate; and the portion of it refracted by the plate is $\frac{1}{2}v^2(1-k^2)^2$, the portion reflected being $\frac{1}{2}v^2k^2(1-k^2)$. These portions include all the rays, however internally reflected, which have only been once reflected by the third surface. The reflected portion $\frac{1}{2}v^2k^2(1-k^2)$ falls on the third surface; and

$\frac{1}{2}v^4k^2(1-k^2)$ is reflected back to the plate, while

$\frac{1}{2}v^2k^2(1-k^2)(1-v^2)$ is refracted through the surface; similar reflections and refractions take place; and the whole intensities of the refracted beams will be

$$\begin{aligned} & \frac{1}{2}(1-k^2)(1-v^2)\{1+v^2k^2+v^4k^4+\&c.\} \\ & = \frac{1}{2}(1-k^2)(1-v^2)\left(\frac{1}{1-v^2k^2}\right); \end{aligned}$$

of the reflected beams will be

$$\begin{aligned} & \frac{1}{2}v^2 + \frac{1}{2}(1-k^2) \cdot \{v^2+v^4k^2+v^6k^4+\&c.\} \\ & = \frac{1}{2}v^2\left\{1+(1-k^2)^2\left(\frac{1}{1-v^2k^2}\right)\right\}. \end{aligned}$$

Of the portion $\frac{1}{2}(1-k^2)$ which falls on a second refracting plate, the intensity of the reflected portion will be

$$\frac{1}{2}k^2(1-k^2),$$

and of the refracted portion

$$\frac{1}{2}(1-k^2)^2.$$

In these are included all the rays, however internally reflected, which have only been once reflected at the outside of the first surface of the second plate.

The portion again reflected by the first plate is

$$\frac{1}{2}k^4(1-k^2),$$

and the portion refracted is

$$\frac{1}{2}k^2(1-k^2)^2$$

This latter portion forms a part of the total beam reflected by the two plates.

Summing up the reflected and refracted portions, we get the intensity of the reflected beam

$$\begin{aligned} &= \frac{1}{2} k^2 + \frac{1}{2} (1 - k^2)^2 \cdot \{1 + k^4 + k^8 + \&c.\} \\ &= \frac{1}{2} k^2 \left\{ 1 + \frac{1 - k^2}{1 + k^2} \right\} = \frac{1}{2} \frac{2k^2}{1 + k^2} = \frac{2v^2}{1 + 3v^2}. \end{aligned}$$

The intensity of the refracted beam is

$$\frac{1}{2} (1 - k^2)^2 \cdot \{1 + k^4 + k^8 + \&c.\} = \frac{1}{2} \cdot \frac{1 - k^2}{1 + k^2} = \frac{1}{2} \cdot \frac{1 - v^2}{1 + 3v^2}.$$

We may in the same way determine the intensities of beams reflected from successive plates; and it will readily be seen that the intensity of the reflected beam is

$$\frac{1}{2} \cdot \frac{2mv^2}{1 + (2m - 1)v^2},$$

and the intensity of the refracted beam

$$= \frac{1}{2} \cdot \frac{1 - v^2}{1 + (2m - 1)v^2},$$

where m is the number of plates.

If we consider the beam polarized in the plane at right angles to the plane of incidence, the reflected portion will be

$$\frac{1}{2} \cdot \frac{2mw^2}{1 + (2m - 1)w^2},$$

and the refracted portion

$$= \frac{1}{2} \cdot \frac{1 - w^2}{1 + (2m - 1)w^2}.$$

The intensity of polarized light in each beam is

$$\frac{1}{2} \left\{ \frac{1 - w^2}{1 + (2m - 1)w^2} - \frac{1 - v^2}{1 + (2m - 1)v^2} \right\},$$

the intensity of natural light reflected being $\frac{2mw^2}{1 + (2m - 1)w^2}$, and

refracted being $\frac{1 - v^2}{1 + (2m - 1)v^2}$.

Let p and n represent the proportions of polarized and natural light in the refracted beam, then

$$\begin{aligned}
\frac{p}{n+p} &= \frac{\frac{1-w^2}{1+(2m-1)w^2} - \frac{1-v^2}{1+(2m-1)v^2}}{\frac{1-w^2}{1+(2m-1)w^2} + \frac{1-v^2}{1+(2m-1)v^2}} \\
&= \frac{2m(v^2-w^2)}{\{1+(2m-1)v^2\}(1-w^2) + (1-v^2)\{1+(2m-1)w^2\}} \\
&= \frac{2m \sin^2(\phi - \phi_1)}{1+(2m-1)v^2 + \cos^2(\phi - \phi_1)\{1+(2m-1)w^2\}} \\
&= \frac{m \sin^2(\phi - \phi_1)}{1+(2m-1)v^2 - m \sin^2(\phi - \phi_1)}
\end{aligned}$$

or

$$= \frac{m \tan^2(\phi - \phi_1)}{1+(2m-1)w^2 + m \tan^2(\phi - \phi_1)}.$$

These expressions may conveniently be put in the form

$$\frac{p}{n+p} = \frac{m}{\frac{1}{\sin^2(\phi - \phi_1)} + \frac{(2m-1)}{\sin^2(\phi + \phi_1)} - m},$$

or

$$\frac{n+p}{p} = \frac{\frac{1}{\sin^2(\phi - \phi_1)} + \frac{2m-1}{\sin^2(\phi + \phi_1)} - m}{m}.$$

For a given value of m (as, for instance, when there are four plates) this expression for the degree of polarization may be readily calculated; and, as we have seen, $\frac{p}{n+p}$ expresses the proportion of polarized light in the incident beam when the light is completely depolarized by the plates.

XXVIII. *Investigations on the Electrophorus.*

By W. VON BEZOLD*.

SOME time ago Professor Beetz called my attention, in conversation, to the circumstance that the experiments on the electrical deportment of an electrophorus-cake do not always succeed with that certainty which might be expected from an apparatus whose functions are supposed to be thoroughly known. As just at that time I was engaged on my recently published experiments on the electrical discharge, and as I had at hand the sensitive mixture of powdered red lead and sulphur, it at

* Translated from a separate impression, communicated by the Author, of a paper read before the Royal Bavarian Academy of Sciences, July 2, 1870.

once occurred to me to use this mixture in testing the electrophorus-cake. I was thereby convinced, not only that this powder furnishes an excellent means of investigating the working of the apparatus, but also that this working is far from being so completely understood as is generally assumed.

The only theory of the electrophorus corresponding to the present state of science is that of Riess*; and it is this which we find in all the text-books of physics. The essential part of this theory is that during the rubbing of the cake three layers are formed—two of the same kind on the two surfaces, and one electrically opposite in the interior. Of these, one is supposed to pass to the form, so that only the two opposite electricities remain upon the cake; by their cooperation all the phenomena may be explained on known laws.

This theory contains two very doubtful points. First, the experiment upon which Riess bases his assumption of three layers may just as well be explained in a different and, indeed, simpler manner. Secondly, it is not very easy to understand why electricity should pass from the cake to the form, and not between the cover and the cake.

In the sequel it will first of all be shown that the first-mentioned experiment must indeed be differently explained. A new theory, prompted by the experiments, will be proposed, by which the second point will also be readily explained.

I must, however, first of all briefly discuss the methods which may be used in such investigations, in order to explain the significance of the powder mixture for this purpose.

§ 1. To investigate the deportment of the individual parts of an electrified insulator two means have heretofore been principally in use. Either the body has been laid directly upon an electroscope, or, when this was not possible, a proof-plane was used for transference. The results thus obtained must be used with great caution if they are not to lead to false conclusions.

Assuming that, after having laid an electrified body (for instance, an electrophorus-cake) on the cap of an electroscope, a positive deflection has been obtained, we can by no means conclude that there is positive electricity at the place in the body investigated. Such a deflection only teaches that at the place in question *negative* electricity is attracted and positive repelled. If the divergence continues even after the removal of the body, either positive electricity has passed to the electroscope or negative to the body.

Hence by the electroscope we only ascertain the direction of the force-components acting at the point in question. The use of the proof-plane may lead to still greater errors. It may be

* *Die Lehre von der Reibungselektricität*, vol. i. pp. 291–305.

used in two ways; either first the body to be tested is touched with it and then the electroscope, or when first in contact it is momentarily connected with the earth. It may happen in the first case that the disk when raised has no electricity, even when at the place touched there was some, or quantities otherwise distributed have really exerted a decomposing action at the place in question. Here the question is solely and simply to know whether the force at work between the insulator and the proof-plane is strong enough to permit electricity to pass between the two. Only when this is the case can an electroscopic indication be obtained, which, however, gives nothing more than the direction of the force which acted at right angles to the proof-plane.

The other mode of testing with the aid of the disk is principally applicable when the forces at work are too small to permit a passage between the body and the disk. The repelled electricity is then removed by the earth-connexion, and only the attracted remains, and then gives a deflection on the electroscope. If, however, the action upon the disk was too powerful, the thin layer between the body to be tested and the proof-plane is traversed by sparks, and afterwards no indications at all, or at most too feeble, are obtained. Of course in this case, too, we only acquire information as to the direction of the force, without any indication as to its locality. Conclusions as to the magnitude of this force are entirely untrustworthy; for we can never be certain whether in the present case the thin layer of air has acted as a complete insulator, or whether it has been traversed by sparks.

But, apart from this uncertainty, testing with the earth-connected proof-plane is accompanied by another great evil. On such a disk the value of the potential function is always $=0$. If now electricity is only diffused on non-conductors (that is, bound to solid points), by approaching such a disk, not the distribution of the force, but its direction is everywhere altered. On the contrary, if electricity is also distributed upon conductors, the arrangement of these quantities of electricity is materially altered by the approach of the proof-plane. Hence all the data which are obtainable, even in the most favourable cases, with the aid of such earth-connected planes only refer to the greater or less modification of the system of forces by the presence of the plane. More favourable are the circumstances when the sensitive mixed powder is used as the test.

It is true that primarily only the direction of the component normal to the surface is given by its means; that is, we know that at the places covered with the yellow sulphur negative electricity is attracted towards the surface, and positive at those

covered with red lead. But in this case we have the incalculable advantage that this result is not obtained merely for one single point as by the direct application to the electroscope, or as the mean value of a larger surface is obtained with the plane, but that the electrical deportment of each individual point of extended surfaces may be seen at a glance. Moreover the peculiar arrangement of this powder enables us to draw a tolerably safe conclusion as to the seat and the origin of the quantities of electricity in operation.

A rubbed surface after being sprinkled exhibits bands which indicate the direction of the rubbing. If electricity has passed to the disk by a spark, peculiar dust figures are obtained; and after glimmering discharges, spots of dust. If, however, it is a case of action at a distance, larger portions of the surface are found uniformly covered with one and the same powder. The least action exerted by the disturbing influence of adjacent bodies (a point, &c.) is at once perceptible to the eye; and any one who will take the trouble to repeat with a good (polished) ebonite plate the experiments subsequently described, will be astounded at the simplicity and precision of the means mentioned and at the beauty of the phenomena. He will also at the same time be convinced that the results obtained with the means hitherto employed, just on account of such local disturbances, must always be liable to great uncertainty.

From this point of view the following very instructive experiments may be made, which afford an excellent opportunity of studying the points here mentioned.

If on one surface of an ebonite plate which rests upon insulating supports, and is beyond the sphere of action of points, a positive spark is passed by means of a needle, after sprinkling, a yellow star is obtained upon the upper surface. Upon the lower one, on the contrary, a yellow spot with indistinct edge is obtained, the magnitude of which is about equal to that of the star. If near the lower surface there was a point or a flame, an indistinct red spot is observed on this surface. If, finally, the plate lay upon a metal plate in connexion with the ground, after the sprinkling on the lower surface a well-defined red spot is formed, the extent of which is less than that of the positive star—that is, a negative Lichtenberg's figure. In the first case electricity was only active on the upper surface, which indicated also on the lower surface its presence by action at a distance only. In the second, negative electricity had really passed to the lower surface, but only by a glimmering discharge, in the third, however, by a spark-discharge.

If an insulating surface to which a powerful discharge passes be covered with a perfectly unelectrical insulating plate (ebonite

or glass), and if the latter be sprinkled, a yellow or a red spot will be obtained which will have approximately the same extent as the positive or negative figure formed upon the lower plate. If the cover be raised before the sprinkling, no trace of such a spot will appear upon it. The first case, then, is only the result of an action at a distance.

Besides the means hitherto mentioned, there is another which in many cases yields very beautiful results. We may investigate the magnitude and direction of the action at a distance in the vicinity of the body to be investigated. We may then draw a conclusion as to the arrangement of the masses in action, as is done in the case of terrestrial magnetism. For this purpose I constructed a small shellac needle 4 centims. in length, at each end of which was a pith ball, and which, like a torsion-balance, was suspended by a cocoon-thread. One ball was negatively and the other positively charged; and it behaved in respect of electricity like a magnetic needle in reference to magnetism. From the centre of the needle a very light plumb was hung (a cocoon-thread loaded with a minute weight), nearly reaching the plate, which was provided with a network of squares 5 centims. in the side. While now the plumb was brought as accurately as possible over a corner of this net, the direction of the needle could be determined with tolerable accuracy by sighting, and thereby the direction of the horizontal components. Observations of the vibrations now gave their magnitude.

A comparatively small number of such observations places one in a position to construct a series of level-surfaces which give the most interesting conclusions.

In this investigation I have used this means also, but, for want of space, must defer the communication of the interesting results for more complete publication in another place. For the present, it may be sufficient to remark that these results are in complete accordance with the theory here developed.

§ 2. This being premised, I will turn to the chief point of this investigation—to the question of the three layers of electricity in the cake of an electrophorus.

Against the assumption of these three layers, objections to the same effect have been urged in another quarter since I commenced the present investigation. Poggendorff, in a paper "On the manner in which Non-conducting Substances are acted on inductively," expresses the view that we must conceive the influencing of non-conductors transferred to the surface—a view which seems to me perfectly correct in the case of the influencing of such bodies. If in the sequel I do not adopt this view, it is only because the facts may all be explained as a simple case of action at a distance, and it is unnecessary to assume that the insulator

or its surfaces are influenced. Moreover my entire theory may be altered so as to fit in with Poggendorff's point of view, and a decision between the two does not seem possible as long as we have no clear conception of the manner in which electricity passes to an insulating-surface.

The experiment on which Riess* bases his assumption of the three layers in the electrophorus-cake is the following:—

If a resin (shellac or ebonite) disk held in the hand be rubbed, when tested by an electroscope it will exhibit negative electricity on both surfaces. If, however, the disk be rubbed while it is placed on a metal plate, the rubbed surface (A) will indicate negative electricity, but the lower (B) none at all†. If now the negative electricity of the rubbed surface (A) be removed by passing it through a flame, the positive electricity of the lower surface (B) can be at once recognized, and the upper surface (A) will appear unelectrical. If the lower surface (B) be then passed through the flame it will be unelectrical, and the upper one negative. By repeating this, first one and then the other surface may be alternately made unelectrical.

These experiments are quite correct, and may be very beautifully repeated with the powder-mixture; we shall subsequently revert to them, when all the experiments relating to the theory of the electrophorus have been described in their due connexion.

To complete this series of experiments, it must be added that, instead of laying the cake while it is rubbed upon a metal plate, it may just as well be held in the hand and the unrubbed surface afterwards passed through a flame. From these experiments, Riess concludes that there are three electrical layers in an electrophorus-cake rubbed while it is held in the hand. This assumption is quite superfluous. Remembering the well-known fact that the action of electricity at a distance is the less altered by the interposition of an insulator the more perfect this insulator, it is intelligible that a cake of such a material, after electrifying one side, must exhibit exactly the same phenomena, even if no other force is at work than that action at a distance. For while by applying the rubbed side A the negative electricity produced by friction passes directly to the electroscope, by applying the surface B the positive electricity excited by induction in the electroscope will pass to B, and the electroscope will therefore also diverge with negative electricity.

If the disk lies during the rubbing upon a metal plate, in consequence of the action at a distance exerted by this rubbed surface, *in this plate* the electricity is separated, and positive elec-

* *Die Lehre von der Reibungselektricität*, vol. i. p. 294.

† This, however, is only the case if the rubbing was strong enough. When feebly rubbed the disk acts just as if it were merely held in the hand.

tricity passes to the surface B in the form of sparks. But this quantity of electricity is insufficient to overcome the action at a distance of the negative electricity of A, and hence it will not be recognizable by the electroscope. Indeed, on the average, as we shall afterwards see, the action of the primarily excited negative electricity will still somewhat predominate. Investigated by means of the powder, the positive star is seen upon B—not, however, yellow on a neutral ground, but *black* (that is, free from dust) on a red ground, if the disk was dusted while held in the hand, or, still better, laid upon high insulating supports. That is, the action of the primary negative electricity prevents the negative sulphur from lying upon the places covered with positive electricity; and the presence of such places is only evidenced by the *smaller attraction* which they exert towards the red lead. If the action at a distance of the primarily excited electricity be diminished by placing the disk with the rubbed side upon a plate in connexion with the earth, yellow stars will appear immediately after the dusting.

The experiments with the flame may be explained in an entirely analogous manner. If, for instance, the unrubbed side B be passed through the flame, it is readily seen that on this side positive electricity must be accumulated, even if it be assumed that there was previously no electricity upon it, and that its electroscopic indication was only occasioned by the action at a distance of the electricity primarily excited upon A. We may even suppose that the decomposition by induction is transferred to the flame itself, or to the layer of precipitated vapour which is everywhere formed on the plate when the flame is passed over it. But in that case negative electricity must be removed by the action of points of the flame, while the positive electricity remains upon the surface.

If now the surface A is treated with the flame, the negative electricity present can only partially be removed, since it is mostly bound (if I may be allowed the expression) by the positive of the surface B. Of course the positive electricity now present upon B preponderates; and thus, as Riess states, by alternately passing the surfaces through the flame, first one and then the other electricity will preponderate, of course with continually diminishing intensity. Making the experiment with the mixed powder, if first of all only a small place upon a plate of ebonite is rubbed, we see red and yellow spots alternately formed upon the corresponding sides.

In order to explain the positive electricity which appears upon B after it has been laid upon an earth-connected plate, or has been once treated by the flame, Riess assumes that there is a positive layer in the interior. Hence, to explain the last-named fact, he

ought to have assumed a series of alternately positive and negative layers in the insulator.

It has hitherto only been shown that the facts observed by Riess *may* be explained otherwise than by the assumption of three layers. It now remains to prove that they *must* be otherwise explained. This may be done with the aid of an experiment which is quite analogous to one described in the paper in Pogendorff's *Annalen* already cited. If the disk be electrified while it is not lying on the bottom plate, and if it be treated with the flame first of all upon the rubbed and then upon the unrubbed side of the plate, on Riess's hypothesis the positive layer must predominate, which is said to have been in the interior of the insulator. On my view, on the contrary, the plate should be quite unelectrical.

Experiment shows, in fact, that the plate really loses all electricity. But it must be made with great caution. I could only get it perfectly clear by taking a larger ebonite plate (at least 25 centims. in diameter), and by only rubbing this in one small place in the middle. When a greater part of the surface was electrified or a smaller plate used, it could not be avoided that some positive electricity should pass from the tips of the fingers of the hand in which it was held, from the sleeves, &c. to the unrubbed side; and thus the experiment became indecisive. An investigation with the powder-mixture makes evident all such disturbing influences.

§ 3. The experiments are now to be described which serve as basis for the new theory, which is founded only on electrical action at a distance. A few repetitions could not here be avoided, as they were indispensable for the comprehension of the whole. Two circular ebonite plates were used for the experiments. One was 5 millims. in thickness, and its diameter was 45 centims.; when used as an electrophorus-cake, it was placed upon a zinc disk 52 centims. in diameter, and had a cover 45 centims. in diameter. The other plate was only 4 millims. in thickness, and its diameter was 23 centims. Experiments were moreover made with plates of ordinary green glass, and identical results obtained—of course, with the opposite sign.

The larger of the two ebonite plates had already been in use for a year as an electrophorus-cake, and in accordance with this was ordinarily protected by its cover. Oddly enough, at the outside edge of this plate, where it was exposed to the light, a breadth of 5 centims. exhibited a totally different behaviour from that of the central part. The small plate was quite new, and in its entire extent behaved exactly like the central part of the other. As I convinced myself both in this manner and otherwise that that peculiar deportment of the edge had its origin

only in an alteration of the surface, and not in the nature of the electrophorus, I take no account of it in this preliminary notice. The descriptions refer only to a new plate, or to the part of an old one which has been protected by the cover.

With these plates the following experiments were made:—

First Experiment.—If the cake be rubbed while supported vertically on a table and only held loosely at the upper edge, after the dusting it will be found covered on both sides with red oxide of lead. The two surfaces present nevertheless different appearances. The rubbed surface exhibits streaks, from which the direction of the rubbing may be inferred, occasionally intermingled with yellow places. On the other side, on the contrary, the powder is tolerably uniform. With stronger rubbing the passage of positive electricity to the plate cannot be avoided, as can be readily ascertained after sprinkling with the powder.

The explanation of this experiment has been given above. Here it is a case of a single negatively electrical layer, and hence positive electricity is attracted on both sides.

Second Experiment.—If the plate be feebly rubbed while it is lying upon the form (*abgeleitete Bodenplatte*), after being lifted up and dusted it acts just as if it had been rubbed in the air. If now the cover be laid upon such a cake resting in the ordinary manner upon the form, a positive spark may be taken from the cover which has been lifted off. But if the cake be inverted so that it lies with the rubbed surface upon the form, the cover after being removed gives either no electricity at all, or at most only traces. If, however, the cake, while its rubbed side is still turned towards the plate, be laid upon insulating supports (for instance, rods of sealing-wax), positive electricity is obtained from the cover when it is laid upon it and removed in the ordinary manner, and the more the higher the supports are.

These experiments show that, with very feeble primary electrifying, electricity passes neither between the cake and the form nor between the cake and the cover, and therefore that in this case only that which is directly excited can come into play.

If now the cake rests in the normal position on the plate, the action of the originally excited electricity upon the cover is but little enfeebled by the positive electricity attracted on the far more distant form, and hence the cover when raised must exhibit distinct quantities of positive electricity. If, on the contrary, the cake has its rubbed side turned towards the form, owing to the positive electricity collected in the closely adjacent form, the action of the originally excited electricity upon the far more distant cover becomes extremely small, and hence when raised the cover will be found to be unelectrical.

A simple calculation shows that the quantities of electricity in

the two positions upon the cover must be as D to d^* ; D being the thickness of the cake, and d the thickness of the layer of air between it and the form (relatively to the cover). But this ratio $\frac{D}{d}$ is in any case a very large number. If, however, the cake is removed from the form, the positive electricity collected upon the form loses its influence, and to a greater extent the higher the supports; the primary again begins to operate, and the cover must therefore yield positive electricity.

Third Experiment.—If while the cake lies on the form it be pretty strongly rubbed, after dusting the rubbed surface there will in general be no essential difference from the previous case. Only when the rubbing is too strong, which may be recognized by a crackling sound when the cover is laid upon the cake, can a circle of yellow rays and stars be observed in those parts over which was the edge of the cover. We will first of all neglect this case, and assume that the upper surface presents the appearance previously described; it will now, on the contrary, be found that the lower surface exhibits a highly remarkable and for the most part very beautiful image, and one which differs according to whether the cake during the dusting (with the surface A) was on the form or on high supports.

In the former case the entire surface is covered with yellow stars which have partially a red central spot; in the latter case no sulphur sticks to the surface, but the same stars appear black (that is, free from dust) upon the surface. If the dusting has been effected in the first position, and if then the cake be raised, the sulphur flies from the stars towards the edge.

This experiment teaches that during the rubbing the space between the form and the cake is traversed by sparks, and the positive electricity strikes on the cake in the form of the well-known stars.

But the quantity of this positive electricity is far smaller than that of the negative which is on the surface A , because, when the latter is not bound by the positive electricity attracted in the form, the action of the primarily excited electricity preponderates; for after the raising the sulphur is not attracted, but repelled, by those parts, which are undoubtedly covered with positive electricity.

This affords an excellent example how the electricity present at a definite place may be made electroscopically imperceptible by the stronger action at a distance of quantities elsewhere distributed.

The accuracy of the opinion above expressed may be tested by a further experiment; for if it is true that the positive elec-

* More accurately as $D+d$ to d .

tricity which has passed to the lower surface can only come into operation when the stronger electricity originally excited is bound, after inverting the cake negative electricity can only so long occur in the cover as the cake is near enough to the form, while with increasing distance of the cake a position must be reached at which the sign of the spark taken from the raised cover changes. That this is the case is seen from the following experiment.

Fourth Experiment.—If the cake of an electrophorus which has been strongly rubbed in the normal position be inverted and laid with the rubbed side upon the form, the cover after being touched and raised will yield negative electricity. But if the cake be laid upon gradually higher supports, the quantity of negative electricity furnished diminishes with extraordinary rapidity, disappears completely at a certain distance between the form and the cake, while at greater distances gradually stronger positive charges occur. This can be admirably seen if, instead of an electroscope, dust-figures are again used, if the cover after each time of lifting be brought into contact with a conductor placed upon a test-plate; for then continually decreasing negative figures are obtained, followed by continually increasing positive ones.

Fifth Experiment.—The experiments previously instituted have shown that when the electrifying is not excessive, no electricity passes between cover and rubbed surface, as indeed has always been assumed, while it does pass from the unrubbed surface and the form. This remarkable fact will be more easily comprehended from the following experiment.

If the cake be rubbed while it lies upon insulating supports, and if it be then covered with an entirely unelectrified ebonite or glass plate, and if then a conductor be placed upon this latter—after dusting, positive figures will be seen upon this plate. These become much smaller if the cake be laid upon the form. The conductor might also be placed directly upon the cake; but this would in general yield no trustworthy results, since the shape of the figure formed upon the part rubbed always depends on the very varied excitation of the individual places.

This experiment teaches that by the vicinity of the form, and of course also by the positive electricity deposited upon the surface B, the decomposing force which A can exert upon a higher point is diminished. Hence a transition of electricity takes place more readily between the cake and the form than between the cake and the cover, which is only subsequently placed upon it; for the electricity originally excited is for the most part bound by that on the form and the surface B. If the original excitation was too powerful, the space between cover and cake

may always be traversed by sparks ; part of the originally excited electricity is thereby neutralized, and we have the original case. Hence it is that for each electrophorus there is a maximum action, depending on the condition of the air, which cannot be exceeded by any rubbing, be it ever so strong. All this may indeed be deduced from the known fundamental principles of electricity ; it nevertheless seemed desirable to confirm this by a special experiment.

From the experiments here adduced it follows that all phenomena observed on the electrophorus may be explained by action at a distance, and that it is unnecessary to have recourse to the assumption of an inductive action in the insulator. All these explanations may readily be put into a mathematical form. This will be done in the more complete paper, in which much of the experimental detail will be discussed. Here I was only concerned to produce in their connexion the most essential experiments, and to give their explanation in general outline.

Briefly summed up, we may say that the process in the ordinary mode of using the electrophorus is as follows :—

The electricity produced by rubbing the upper surface of the cake acts inductively upon the form.

If the original excitation is strong enough, the (opposite) electricity of the form traverses the space between the latter and the cake, and passes to it, forming spark discharges.

Both by this electricity which has passed, and by the residual electricity in the form, the primarily excited of the upper surface of the cake is partially bound.

The force which is operative in the space between the subsequently superimposed cover and the cake is by this diminished, and an exchange of electricity in this space is prevented.

The electricity of the opposite kind produced in the cover by the induction of that primarily excited remains upon it, and by conducting off electricity of the same kind and raising the cover becomes free—that is to say, is made electroscopically active.

All other accompanying phenomena may be explained from these points of view according to known laws.

XXIX. *On the Corona seen in Total Eclipses of the Sun.*

By Professor W. A. NORTON*.

IN a former communication † I alluded briefly to the auroral theory of the solar corona, and referred to publications in which I had advocated it. I propose now to give a brief discussion of the theory.

* From Silliman's American Journal for January 1871.

† Silliman's Journal for September 1870.

The grounds upon which I have maintained the auroral origin of the corona in different publications are the following :

1. The corona cannot be the permanent atmosphere of the sun, shining by reflected light, since its outline is neither circular nor oval, but exceedingly irregular, and it extends out from the sun many times further in some directions than in others. The utmost that can reasonably be maintained is that for a small portion of its outward extent, for which the gradation of light is nearly uniform, it may possibly be a solar atmosphere.

2. The natural indications of the aspect of the corona are that it is chiefly composed of separate masses of luminous matter, of unequal brightness and length, radiating out from different points of the sun's limb. The general radiated structure of the corona, and the great comparative outward extent of the luminous radiations in certain directions, have attracted the attention of the observers of all modern eclipses. Some streamers have been seen to extend more than 1,000,000 miles from the sun, while others did not extend to one quarter of this distance.

3. Reasoning analogically from the earth to the sun, we naturally conceive the body of the sun to be surrounded by a permanent atmosphere. On the same grounds we should infer that the space exterior to this atmosphere is pervaded, either occasionally or permanently, by auroral streamers, similar to those which at times shoot out many hundreds of miles into space from the upper atmosphere of the earth.

4. If the luminous radiations of the corona are in fact auroral streamers, we must expect that they will not be permanent in their extent and position. Now it is well known that such is the fact ; for the aspect of the corona has been very different in different eclipses (*e. g.* eclipses of 1842, 1851, 1858, and 1860). It has even been maintained by some observers that the rays of the corona had a flickering lustre, and varied in extent and position during the short period of a single eclipse.

5. Admitting, as we must, the actual radiated structure of the corona, its individual streamers, or luminous radiations, may be conceived either to be permanently connected with the sun, or to be composed of luminous matter actually streaming away from the sun, to an indefinite distance, into space. If we adopt the former idea, we virtually admit that a permanent vaporous atmosphere of sensible density extends from the body of the sun to a distance greater than the sun's diameter—a position that cannot with any plausibility be maintained*. In sup-

* According to the recent spectroscopic determinations of Lockyer and

port of the other hypothesis we have the well-established fact that some form of luminous matter, belonging to cometary bodies, when it comes under a certain degree of influence from the sun, is projected or in some manner detached from the nuclei of these bodies, and repelled from the sun, and under the operation of the solar repulsion urged away from them to an indefinite distance, forming the luminous trains by which they are attended. (See the author's papers on Donati's Comet published in *Silliman's Journal*, Jan. and May, 1860, and July, 1861; and the discussion of the Dynamical Condition of the Head of a Comet in the *Number* for Jan. 1859). To suppose that the rays of the corona are actual radiations of luminous matter, is only to suppose that a portion of the photospheric matter of the sun becomes subject to the operation of the same forces that we perceive the sun to exert upon a portion of the matter of comets. The luminosity of such radiations may be ascribed either to a reflection of the sun's light, or to electric discharges. Upon this question, we shall see, important evidence was obtained at the total eclipse of August 7, 1869.

6. If we adopt the auroral theory of the corona, and at the same time admit that the auroral streamers are actual emanations of luminous matter, the following consequences may be expected to follow.

(1) A portion of the auroral matter emitted from the sun should fall upon the earth's atmosphere, and may furnish the substance of terrestrial auroras, for which no terrestrial origin has yet been detected.

(2) Upon this view of the possible origin of terrestrial auroras, the close correspondence that has been detected between the periods of the sun's spots and of auroras should subsist, if we allow that the spots are merely the natural result of the supposed discharges of the solar matter, prevailing for a time at certain points of the photosphere—or, indeed, if we grant that they are in any way the result of these discharges visible in the corona.

(3) In the wave-propagation of the impulsive actions on the ether of space of the electric discharges to which we may ascribe the material emanations from the photosphere, and in the electric and magnetic phenomena attendant upon the reception and accumulation of the solar auroral matter in our atmosphere, we have a plausible general explanation of the periodic and irregular disturbances of the magnetic condition of the earth, and of

Frankland, the solar atmosphere must be of exceeding tenuity in the region of the rose-coloured protuberances, just above the general surface of the chromosphere.

their known physical relations to the sun's spots. This theory of the origin of the diverse perturbations of terrestrial magnetism I have elaborated, and followed out into a detailed discussion of the variations of the different magnetic elements, in former Numbers of Silliman's Journal (viz. for March and July, 1855).

We may add that it derives additional support from the general result arrived at by Professor Chambers, in his discussion of the "Nature of the Sun's Magnetic Action," viz. that "the mode in which forces originating in the sun influence the magnetic condition of the earth is not analogous to the action of a magnet upon a mass of soft iron placed at a great distance from it, but that these forces proceed from the sun in a form different from that of magnetic force, and are converted into this latter form of force probably by their action upon the matter of the earth or its atmosphere." If this be admitted, then we must conclude that the perturbations of the earth's magnetic condition, as evinced by the variations of the position and directive force of the magnetic needle, must result either from some action, direct or indirect, on the earth or its atmosphere, of some form of matter emitted from the sun, or from a wave-action propagated from the sun, or from both of these operative causes combined.

(4) The streamers of the corona should have at different points of the sun's photospheric surface different directions, parallel to the diverse directions of the magnetic force of the sun at this surface. These directions should be variously inclined, in different heliographic latitudes, to the horizontal lines at the points of the surface, and also to the plane of the sun's equator; like the dipping-needle on the earth and the streamers of a terrestrial aurora. In low latitudes the angles of inclination to the plane of the equator should be large; and the streamers proceeding from corresponding points in the two hemispheres should converge and intersect in the plane of the equator. In proportion as these corresponding streamers proceed from points more remote from the sun's equator, they will intersect under a smaller angle, and their point of intersection will be more distant from the sun's surface, until at the heliographic latitude of 30° to 35° they will become parallel to the plane of the equator. Those emanating from still higher latitudes will diverge from the plane of the equator and from each other*.

If these facts be attentively considered, it will be seen that the result should be the formation of a luminous appearance

* It is here assumed that the magnetic equator of the sun is coincident with his heliographic equator.

extending indefinitely outward from the sun into space, and elongated in the plane of his equator, and that to observers on the earth it would have an apparent form more or less triangular.

The same fundamental conception which accounts for the solar corona and the physical relations known to subsist between the sun's spots and terrestrial auroras, as well as between these spots and the varied magnetic disturbances occurring on the earth, furnishes, then, an adequate explanation of the extent, form, and position of the *Zodiacal Light*. In fact we see that the zodiacal light is but the indefinite extension of the corona.

We have here tacitly supposed that the solar emanations consist of magnetic matter projected with great velocity into space in the directions of the prolongations of the auroral columns, and proceeding on indefinitely in these directions; but if, like the cometic matter, they are exposed to a continual repulsion from the sun, the paths described by the receding particles would be hyperbolas convex towards the sun. The point of intersection of any two streamers proceeding from corresponding low latitudes in the two hemispheres, would in consequence be thrown to a greater distance from the sun; but the general result, as to the form and position of the luminous appearance produced (the zodiacal light), would be the same.

In support of this view of the origin of the zodiacal light, we may state that Cassini drew from his observations on the sun's spots and the zodiacal light, made during the interval from 1665 to 1688, the conclusion that a physical connexion subsisted between these two phenomena, and that the substance of the zodiacal light was, in fact, some emanation from the sun's spots. Again, according to Arago, it appears, from the entire series of observations at Paris and Geneva, that the zodiacal light varies considerably from one year to another, and that the observed variations cannot result entirely from changes in the transparency of the atmosphere. We shall soon see that the form of the corona, as seen in the eclipse of 1869 and previous eclipses, presented certain prominent features that accord with the theoretical conclusion that the zodiacal light is but the indefinite extension of the corona.

It is proper to state here that in what precedes we have really been contemplating but different sides of one comprehensive theory, which embraces a connected series of solar phenomena, of which the corona is but one term. The outline of this theory is given in the author's '*Treatise on Astronomy*,' revised edition (1867). It is:—that a portion of the matter of the sun's photosphere is in the habitual condition of auroral magnetic

columns ; that by electric discharges along these columns their substance becomes dispersed and in part projected into space, and that this process, wherever occurring, may, by a continued dissipation of a portion of the photospheric matter at that locality, eventuate in the formation of a visible spot on the disk ; that the photospheric matter thus discharged into space is in that peculiar condition recognized in cometic matter in which it becomes subject to a repulsive action from the sun (or else to a diminished attractive action, as occurred to a certain extent in the case of Donati's comet, and in that of 1861), and in the act of flowing away is visible in solar eclipses as the streamers of the corona, and at more remote distances as the zodiacal light ; that these solar emanations furnish the matter of terrestrial auroras, and, when descending in copious showers into the earth's atmosphere and developing electric currents and disturbing the magnetic condition of the earth, are the determining cause of all the phenomena of "magnetic storms." The apparent structure and variability of the corona, and, as we shall soon see, the most characteristic features of its form, the form, position, and variability of the zodiacal light, the coincidence of the periods of the sun's spots with the periods of terrestrial auroras and with those of the perturbations of the magnetic needle, all accord with this general theory. I have also endeavoured to show, in my papers on the variations of the magnetic elements, that these variations are such as should naturally result from the electric currents in the upper atmosphere (or what may be called the photosphere of the earth) that would ensue from the reception of the supposed impulsive waves and material emanations proceeding from the sun. (See Silliman's Journal, II. vol. xix. March and July, 1855).

If it indeed be true that, from the fundamental conception of material emanations from the sun, similar to those which we know to take place from the head of a comet under the influence of the sun, a connected series of phenomena may be theoretically deduced which have their actual counterparts in nature, it must be conceded that there is a high probability that this conception is founded in truth, and furnishes the true explanation of the varied phenomena observed.

Results of Observations on the Corona made at the Total Eclipse of August 7, 1869.

The observations made on the Corona on the occasion of the eclipse of 1869, have furnished several striking confirmations of the theory that it is an auroral phenomenon.

1. *The observed form and structure of the corona.*—I will first duce the results of my own observations. These were made

at Des Moines, Iowa, with the naked eye and a good opera-glass, and were chiefly confined to the corona. When the totality commenced, and the beautiful corona stood revealed, like a new creation, against the dark background of the sky, almost the first striking feature that caught my attention was the great inequality in the extent of its outstreaming in different directions, and its consequent irregularity of outline. This outstreaming or luminous radiation was particularly conspicuous from the eastern limb, *nearly in the direction of the plane of the ecliptic or the sun's equator*. It could be distinctly traced in that direction to a distance from the sun equal to his own diameter. For an extent of some 15° on either side of the ecliptic, individual hair-like streamers, seemingly nearly parallel to the ecliptic, extended out a large fraction of this distance. From the opposite limb, and in the opposite direction, the coronal streamers were conspicuous, but of less extent than in the direction of the ecliptic toward the east. From the polar regions other pointed masses of light extended out to considerable distances, but not so far as those just noticed. They seemed to be composed, like the others, of rays or hair-like luminous radiations, more or less distinct. The separate luminous lines appeared to Professor Eastman, from the United States Naval Observatory (who observed the corona at the same station through a small telescope), to converge more or less. This convergence I failed to detect; but I distinctly noticed that the outstreaming mass from near the north pole of the sun had approximately the form of a triangle with curved sides, convex outward; but the triangular outline appeared as if resulting from the intersections of individual radiations, rather than as being the definite boundary of a stationary luminous mass.

The corona had a white silvery lustre, and appeared at times suffused with a delicate rosy tinge; but this was probably a subjective effect. No flickering or variation of the lustre of the corona was observable during the totality. Nor was there any noticeable change in its general form, or in the extent of its luminous radiations, though I carefully watched for such changes. Dr. B. A. Gould, who was stationed at Burlington, Iowa, and other observers, thought that both the lustre and extent of the radiating masses, or "star-points," underwent material variations. A similar difference of opinion is found in the reports of observations on previous eclipses. If variations in the brightness and extent of the coronal radiations do actually occur, it is favourable rather than opposed to the auroral theory of the corona; but it is probable that the apparent changes are due to inequalities in the interceptive action of the earth's atmo-

sphere on the light of the corona, and especially on the faint light at its outer boundary.

Professor Harkness, of the U. S. Naval Observatory, in his able report of observations on the eclipse, states that the four angles of the trapezoidal outline of the corona were in the middle heliographic latitudes—relying upon the report of another observer; but on a direct examination of the question of the location of these angles, or “star-points” of the corona, made since the publication of the report, he has satisfied himself that their actual position was such as I have above reported it from my own observations*.

In the delineations of the corona given by the observers of previous eclipses, two or more conspicuous outward extensions are generally shown, but the positions of these more projecting parts are seldom given with respect to the equator or poles of the sun. The figure of the eclipse accompanying the report of P. Prof. Capellotti, of observations on the eclipse of April 15, 1865, made at Chili, is an exception. It shows three principal points of outstreaming of the corona—two lying very nearly in the plane of the sun’s equator, and nearly diametrically opposite to each other, and a third near one of the poles of the sun. In the eclipses of 1858, 1860, and 1868, four such points were seen, distributed at about a quadrant’s distance from each other. In the eclipse of 1842 but two were noticed, which were diametrically opposite to each other. In that of 1851 there appears to have been no marked deviation from a general uniformity of radiation.

Relying, then, upon the only definite knowledge we have of the location of the more conspicuous portions of the corona, viz. that obtained in the eclipses of 1865 and 1869, we may say that the corona is brighter and more extended in about the direction of the plane of the sun’s equator than in any other direction. This striking fact lends a powerful support to the auroral theory of the corona; for, as we have already seen, the streamers proceeding from the lower latitudes on the sun, on opposite sides of the equator, should converge and intersect in the plane of the equator, and for a certain distance on either side of this plane, and in consequence the corona should appear to extend further in the plane of the equator than in other directions. The convergence of individual rays or lines of emanation,

* Professor Winlock, in his report of observations on the eclipse, says, “The photograph of the corona taken at Shelbyville shows a flattening at the extremities of the sun’s axis, and an elevation about the equatorial region.” The photographic impressions obtained of the eclipse at the different stations show, however, but a small portion of the outward extent of the corona visible to the naked eye.

we have already seen, was actually noticed by Professor Eastman. It, of course, may happen that inequalities in the amount of outstreaming on opposite sides of the equator may throw the more prominent and conspicuous parts of the corona to the one side or the other of the plane of the equator. It will be observed that, from our present point of view, the extension of the corona in the plane of the sun's equator is a phenomenon kindred to the much greater luminous extension seen in the zodiacal light—the only difference between them being that in the former the auroral emanations proceed from lower heliographic latitudes, and intersect nearer the sun.

But it may be asked, how are we to explain, on the present theory, the “star-points” of the corona over the polar regions of the sun? For these, two reasons may be assigned:—(1) If we admit a distribution of magnetism on the sun similar to that which prevails on the earth, the auroral streamers should diverge from each other less rapidly in the high than in the low latitudes. (2) Upon opposite sides of a line of no declination traversing the sun's surface, analogous to that which traverses Russia, the natural directions of the streamers prolonged upward would be such as to occasion the convergence and intersections of those proceeding from the opposite sides of this line.

We may say, then, that the more extended portions of the corona, in the eclipse of 1869, were over those regions of the sun's surface, and those only, where upon the present theory the intersections of streamers might be expected to occur.

In some eclipses distinct luminous curves having the appearance of luminous jets issuing tangentially to the sun's limb, or obliquely inclined to it, and pursuing a course either convex or concave to the limb, have been seen. According to M. Liais these peculiarities were conspicuously observable in the eclipse of Sept. 7, 1858. While it is possible that such curves may be the result of the intersections of a mass of straight streamers, it is not improbable that they may be actual luminous jets; for if from any cause any portion of the auroral matter should be projected from the sun in a direction oblique to the surface, it would proceed in a convex hyperbolic curve if repelled by the sun, and in a concave curve if attracted. Now I have shown, in a former number of *Silliman's Journal* (July 1861), that the portion of cometary matter posited on the convex side of the tail of Donati's Comet was actually repelled by the sun, while that on the concave side had become detached from the head of the comet, because of a diminished gravitation toward the sun. Upon our fundamental conception that the coronal matter is essentially in the same physical condition as such cometic matter, and subject to the action of the same solar forces, it may well happen

that some individual jets will proceed in convex, and others in concave curves, accordingly as the escaping matter is repelled or attracted by the sun.

2. *Observations on the physical constitution of the corona with the spectroscope and polariscope.*—The results of the observations made at the late eclipse, with the spectroscope and polariscope, are strongly confirmatory of the truth of the theory of the corona under discussion. Professor Pickering, in the report of his observations with a polariscope, says, “The form of polariscope used was that adopted by Arago in his experiments on sky polarization. It consists of a tube about 20 inches long and 2 inches in diameter, one end of which is closed by a double image-prism of Iceland spar, and the other by a plate of quartz. Looking through the former we see two images of the latter, which, when the light is polarized, assumes complementary tints. If, now, the corona was polarized in planes passing through the centre of the sun (as is generally admitted)—when viewed through the polariscope, in one image the upper and lower parts should have appeared blue, and those on the right and left yellow; while in the second image these colours would have been reversed, the yellow being above and below, and the blue on the sides. In reality the two images were precisely alike, and both pure white; but one was on a blue, and the other on a yellow background. From this we infer that the corona was unpolarized, or, at least, that the polarization was too slight to be perceptible.”

We may infer from this that the corona is either self-luminous or shines by diffuse reflection, since specular reflection produces polarization*.

The testimony of the spectroscope is still more decisive. Profs. Pickering, Harkness, and Young agree that the spectrum from the light of the corona was a continuous one, or free from dark lines, but containing one or more bright lines. The absence of dark lines indicates that the corona did not shine by the light of the photosphere, reflected either diffusely or specularly from its substance; since such light, after reflection, should, like the direct solar light, have given a spectrum with the Fraunhofer lines. The presence of bright lines, on the other hand, is a direct indication that the corona was self-luminous, and therefore that its light was the result either of combustion or of electric discharges. As it is hardly supposable that an actual combustion could prevail at the distance of tens and hundreds

* The question whether the light from the corona is in any degree polarized or not cannot be regarded as definitively settled. It is to be hoped that the observations to be made on the eclipse of December will remove all doubt on this point.

of thousands of miles from the sun's photosphere, in regions where, if any solar atmosphere exist, the results of recent observations with the spectroscope by Lockyer and Frankland lead us to believe that it can only be the faintest possible trace of it, we must infer that the light of the corona is of electric origin.

In the hands of Prof. Young and Prof. Winlock the spectroscope has obtained direct evidence of a physical correspondence between the solar corona and terrestrial auroras. Prof. Young observed in the spectrum of the corona a bright line, the position of which he gives as 1474 on Kirchhoff's scale, and which proves to be in coincidence with a small line marked as *iron* on Kirchhoff's and Ångström's maps. He remarks that "it turns out also to coincide very closely, if (which is much more probable) it is not absolutely identical, with a line recently discovered, by Prof. Winlock of Cambridge, in the spectrum of the aurora borealis. He also saw two other, fainter lines in the spectrum of the corona which coincided quite closely with other lines reported by Prof. Winlock as visible in the spectrum of the aurora. In view of these results of spectroscopic observation he remarks as follows:—"At present it seems pretty likely that the spectra of the corona and the aurora borealis are identical, with only such differences in the intensity of their lines as we might naturally expect, and that very probably the identity extends to the essential nature of the phenomena themselves."

The detection of the same iron line in the aurora and corona, taken in connexion with the well-established fact that the vapour of iron is present in the photosphere and chromosphere of the sun, and that the magnetic features of the aurora lead to the natural conclusion that some form of ferruginous matter constitutes the substance of auroras, for which no terrestrial origin can reasonably be assigned, conducs to the inference that the terrestrial auroral matter is derived from the sun, and adds to the weight of accumulative evidence in support of the theory I have advocated, that the corona is made up of material emanations from the sun.

NOTE.—Some persons have conjectured that the corona might be produced by the passage of the sun's rays through the earth's atmosphere; but it may readily be shown that this is impossible. When one reflects that the half width of the moon's shadow, in the larger eclipses, is as great as the estimated height of the atmosphere, it will be seen that, to an observer on the central line of the eclipse, the line of sight will not fall upon the illuminated portion of the atmosphere exterior to the shadow, unless inclined under a large angle to the line of direction of the centres of the sun and moon. The corona, therefore, if of terrestrial atmospheric origin, ought to present, toward the middle of the eclipse, the appearance of a halo *entirely de-*

tached from the dark body of the moon, and many degrees distant from it. It ought also to increase in brightness from its inner border for a considerable distance outward.

Others have imagined that the corona might be attributable to the passage of the sun's light through a lunar atmosphere; but since some of the streamers, or rays of the corona, have been seen to extend to a distance greater than the sun's diameter, this would require the lunar atmosphere to be of vast extent; whereas no decisive evidence has yet been obtained of the existence of any lunar atmosphere capable of producing a sensible refraction, or reflecting a perceptible amount of the sun's light to an observer on the earth.

Perhaps the more prevalent idea, at the present day, is that the corona, with its rays and tufts of light, is a phenomenon of diffraction produced by the passage of the sun's rays along the denticulated edge of the moon. This theory has an air of plausibility, but it is entirely inadequate to account for the great extent of the coronal rays. The fringes produced by the diffraction of light in its passage near the edge of a body appear to the eye of the observer to extend but a small angular distance from the edge. This would be more strikingly true in the case of a distant body, like the moon.

The only remaining supposition is, that the corona is either an envelope of some kind permanently connected with the sun, or is made up of material emanations proceeding immediately from the sun. To the large body of indirect evidence that has been obtained that the corona is wholly a solar phenomenon, we may now add that of direct observation, since it appears that "an examination of the photographs of totality," obtained at the eclipse of 1869, shows that as the moon advanced the corona was progressively covered.

XXX. Notices respecting New Books.

Principles of Mechanism, designed for the use of Students in the Universities, and for Engineering Students generally. By ROBERT WILLIS, M.A., F.R.S., *Jacksonian Professor of Natural and Experimental Philosophy the University of Cambridge, &c.* Second Edition. London: Longmans, Green, and Co. 1870.

THE distinction between motion considered merely as change of place, and motion as produced by force, was often noticed by the earlier writers on mechanics, *e. g.* by Euler; and the possibility of establishing a distinct science of pure motion was pointed out by more than one writer, and particularly by Ampère, who denominated it *Cinématique* or *Kinematics*. It is, however, worthy of remark that there are two points of view from which such a science may be regarded, viz. either, *first*, in its most general aspect, as a science which treats of the transference of a collection of points from a given position to any other position, and discusses the formulæ which define the positions of these points after and during transference with respect to their initial place; or, *secondly*, as a science which enumerates and classifies the means by which we change the direction and velo-

city of a given motion. If we adopt the nomenclature that *Dynamics* is the general doctrine of Force, and *Mechanics* the applications of that doctrine to Machines, then *Kinematics* stands related to the former of these in the same manner as *Mechanism* to the latter. The fundamental distinction implied in the word kinematics or pure mechanism is now so familiar to students of mechanical science, that we are apt to forget how few years have passed since that distinction was actually effected :—

“The science of pure motion,” says the late Dr. Whewell*, writing, we believe, in A.D. 1840, “has not generally been separated from the science of motion viewed with reference to its causes. *Recently, indeed*, the necessity of such a separation has been seen by those who have taken a philosophical view of the science.” It was mainly by the labours of Mr. Willis that this separation was actually effected, and the theory of pure mechanism established as an independent science. This was done in his well-known treatise ‘The Principles of Mechanism,’ the first edition of which was published in the year A.D. 1841. In noticing the appearance of the second edition of a work which long ago took its place as the standard work on the subject, it is quite unnecessary to speak in its praise. Nor are the changes introduced into the new edition of a kind to call for a long notice. Perhaps the most important is the rearrangement of the first and principal part of the volume. This change is as follows :—One piece of a machine may communicate motion to a second piece either (A) by rolling contact, (B) by sliding contact, (C) by wrapping connexion, (D) by link-work, (E) by reduplication. Under each of these divisions there are three classes :—(1) Directional relations of the motions constant, and velocity-ratio of the pieces constant. (2) Directional relation constant and velocity-ratio varying. (3) Directional relation changing periodically and the velocity-ratio constant or varying. In the first edition the classes were treated separately in the five divisions. In the new edition the divisions are given separately, and each, except (E), subdivided into the three classes. Mr. Willis states that, in his experience as a lecturer, he found the very great convenience of thus classing the elements of machines with reference to the mode in which velocity is communicated. The same arrangement, it may be mentioned, is adopted by Dr. Rankine in part iv. of his treatise ‘On Applied Mechanics;’ and there can be no doubt that it constitutes a great improvement in the form of the work.

Most of the changes in the substance of the book are such as are sure to occur to an author who re-edits a book after a lapse of nearly thirty years: new articles are inserted here and there; additional notes relating to the history of the subject are of frequent occurrence; and besides these there are two parts wholly new, viz. on “Combinations for the action of which properties of friction are employed,” and on “Universal Joints.” The effect of all this has been to increase the book by about one third part of its original size. The revision

* Philosophy of the Inductive Sciences, p. 152 (ed. 1846).

seems to have been made as thoroughly as such a work deserved; and no pains have been spared in making it perfect down to the most minute particular. It had, we believe, been long out of print; and we congratulate students of Mechanical Science on being able to procure what will long be the standard work on this subject.

A Laboratory Text-book of Practical Chemistry; or, Introduction to Qualitative Analysis. A Guide to the Course of Practical Instruction given in the Laboratories of the Royal College of Chemistry. With ninety engravings. By WM. G. VALENTIN, F.C.S. London: Churchill, 1871. Pp. x and 380.

When the preliminary stages in the growth of an idea or an opinion have been passed, it invariably happens that a literary embodiment is found necessary. The great and increasing estimation in which physical science is now held as at once a means and object of education, furnishes many an illustration of this; and, in its turn, physical science has to embody successive opinions and ideas as they arise. Thus a perfect scientific literature would be a diffused history.

Mr. Valentin's text-book is very remarkable (indeed, novel) in one respect. It puts prominently forward, as an element in the teaching of analysis, the most advanced form of the atomic method of symbolization; and it does so expressly on the ground that the study of chemical analysis is thereby simplified in a marked degree. It has hitherto been the custom for writers of analytical works to keep formal chemistry very much in the background, and to regard even equations rather as *impedimenta* than active aids. But some change has long been expected, and was clearly inevitable. When the results of chemical research are almost uniformly expressed, not only in atomic language, but in atomic symbols, and almost every memoir concludes with or contains a graphic formula, it is clearly the duty of some practical teacher of recognized ability to answer by actual trial the important question, Can such formulæ be used as of real assistance to the analytical instructor? Mr. Valentin has made the experiment, and responds in the affirmative.

The first part of the work (128 pages) is taken up with a varied series of one hundred operations, which become successively more complex in their meaning as we proceed. On each occasion the teacher stands, as it were, at the student's side, points out what is to be observed and what is extraneous, and then exhausts the phenomena of their inferences. The entire book is singularly and admirably argumentative in this manner. Much of its first portion would furnish, and no doubt will be made to furnish, material of a high class for the earlier lectures of a chemical course, more especially when we hear in mind the numerous and well-executed engravings by which it is illustrated. Where not original, these have been taken from the best sources. By the time the student has reached Part II. he may fairly be supposed to understand all the ordinary operations of analysis, the management of apparatus and reagents, the transference and manipulation of gases; but he will have, in

addition, as a necessary part of that knowledge, an acquaintance with the laws of the results of chemical action, which he will know how to express intelligently either in simple or structural symbols. He will also possess a cultivated observation, and he will not fail to have become at once more logical and critical. Thus armed, he may undertake the study of qualitative analysis with confidence.

The second Part commences with an account of reagents and group reagents. Of these, the former are given in detail under the respective elements; the latter precede them in their proper deductive order. In the separation of mixtures, every effort appears to have been made to adhere in the main to quantitative methods; in this way the time of both teacher and student is economized, and the latter is assured of the precision of his operations. Basyulous bodies are considered in the first six chapters of the eight of which this division of the book consists; acid substances (organic and inorganic) are treated of in the remainder. A scheme for the analysis of simple and complex substances, both in the dry and wet way, forms a fitting sequel to the whole: the Tables of which it consists are those of the Royal College of Chemistry, where the author is chief laboratory instructor. These have been repeatedly revised and corrected, having been made the basis of special inquiries during the course of several years; they are now published for the first time, and will be warmly welcomed by old collegians. The special feature of this book is, as we have said, the introduction of the graphic method and its principles into analytical training—not, therefore, as an end, but a means to an end. Upon the atomic notions which underlie that method, the author justly refuses to dogmatize. "Indeed," he says (p. 31), "matter itself being as yet unknown to us, need we wonder that the very existence, size, shape, &c. of the smallest particles of matter should not be capable of experimental demonstration? Where experiment fails to throw light upon the absolute nature of the constant chemical combining proportions, and can at best only supply us with relative data, hypothesis steps in." We may agree or disagree with the atomic hypothesis; but in either case we must admit the indebtedness of chemical literature to the writer. Mr. Valentin teaches his student *to reason*—thereby enabling him to acquire, in time, a power of independent judgment, both as to facts, to theories, and the nature of theory.

XXXI. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 71.]

Nov. 24, 1870.—General Sir Edward Sabine, K.C.B., President, in the Chair.

THE following communication was read:—

"On the Theory of Resonance." By the Hon. J. W. Strutt.
An attempt is here made to establish a general theory of a certain

class of resonators, including most of those which occur in practice. When a mass of air or other gas is enclosed in a space bounded nearly all round by rigid walls, but communicating with the external air by one or more passages, there are certain natural periods of vibration or resonant notes whose determination is a matter of interest. If the dimension of the air-space is small compared with the wave-length of the vibration, the dynamics of the motion is, in its general character, of remarkable simplicity. It is for the most part under this limitation that the problem is considered in the present paper. The formula determining the resonant note is

$$n = \frac{a}{2\pi} \sqrt{\frac{c}{S}},$$

where n is the number of complete vibrations per second, a the velocity of sound, and S the capacity of the air-space; c is a quantity proved to be identical with the measure of electric *conductivity* between the interior of the vessel and the external space, on the supposition that the air is replaced by a uniform conducting mass of unit specific conducting-power, and the sides of the vessel and passages by insulators. When there is more than one passage, the formula is still applicable according to the above definition of c ; and when the passages are sufficiently far apart not to interfere with each other, the resultant c is, by the electrical law of parallel circuits, simply the sum of the separate values for each passage considered by itself. When this condition is not satisfied, the value of c , thus, found by mere addition, is too great.

The question thus resolves itself into the determination of the conductivity (or the *resistance* which is its reciprocal) for different forms of passages or openings. The case of openings, which are mere holes in the sides of the vessel, has been already treated, although in a very different way, by Helmholtz, who, in his celebrated paper on vibrations in open pipes, compared his theory with the observations of Sondhauss and others on the notes produced when such resonators are made to speak by a stream of air blown across the mouth. Sondhauss has also given an empirical formula applicable when the connecting passages are of the form of long cylindrical necks. These previous results are in agreement, as far as they go, with the formula here investigated, and which is applicable whatever may be the length of the neck. If L be the length and R the

$$\text{radius, } \frac{1}{c} \text{ or the electrical resistance} = \frac{L + \frac{\pi}{2}R}{\pi R^2}.$$

This supposes the neck a circular cylinder. If the section be an approximate circle of area σ , we may put

$$\frac{1}{c} = \frac{L}{\sigma} + \frac{1}{2} \sqrt{\frac{\pi}{\sigma}}.$$

When the neck is very long, the second term may be neglected; and when L is very small, the first term becomes insignificant. In the third part experiments are described which were instituted to

compare the general formula with observation, and which gave a satisfactory agreement. The value given above for $\frac{1}{c}$ is only approximate. It is proved, however that the resistance of a finite cylindrical conductor whose plane ends lie in two infinite insulating planes, but join on to conducting masses on the further side, corresponds to a length $L + \alpha$ of the cylinder, where

$$\alpha < 2.305 R \frac{10.615 - \epsilon^{-\frac{8L}{R}}}{14.771 - \epsilon^{-\frac{8L}{R}}}$$

$$> \frac{\pi}{2} R.$$

As a particular case, it appears that the correction to the length of an organ-pipe, supposed, as in Helmholtz's paper, to be surmounted at the mouth by a wide flange, lies between $.785 R$ and $.8282 R$.

Approximate formulæ are investigated for the resistance of tubes which are not exact circular cylinders. It will be sufficient to particularize here the case of tubes of revolution. The resistance is shown to lie between the two limits

$$\frac{1}{\pi} \int \frac{dx}{y^2}$$

and

$$\frac{1}{\pi} \int \frac{1}{y^2} \left\{ 1 + \frac{1}{2} \left(\frac{dy}{dx} \right)^2 \right\} dx,$$

where y denotes the radius of the tube at the point x .

When there is more than one vessel in the vibrating system, there are several independent periods of vibration corresponding to the degrees of freedom. The theory of these vibrations is also considered.

In the experimental part of the investigation the object is to determine with sufficient precision the pitch of the resonant note. This is generally done by causing the resonator to speak. For several reasons, which are detailed, I consider this course unsatisfactory, and have availed myself of other indications to fix the pitch, which are not, indeed, capable of so great an apparent precision, but yet are more to be depended on.

XXXII. *Intelligence and Miscellaneous Articles.*

ON SOME ANALOGOUS PRINCIPLES OF PHOTOMETRY, AND THE
LAW OF ATTRACTION. BY W. VON BEZOLD.

IN what follows I shall communicate a few principles of photometry which are perfectly analogous to well-known theorems in the law of attraction. The consideration of these analogies leads, on the one hand, to simple solutions of many photometric problems, and, on the other, to a very manifest illustration of some principles of the doctrine of the potential. As the proofs for the photometric principles are given in just the same manner as those for the law

Phil. Mag. S. 4. Vol. 41. No. 272. March 1871. R

of attraction, we may in the sequel dispense with any strict demonstration. Hence I will briefly state the more important of these principles, adding now and then a short observation.

In reference to the measure chosen, it must be premised that the intensity of that source of light is taken as unit which at the unit distance imparts to a perfectly white element of surface at right angles to the line of junction the unit of brightness. Hence a luminous point of the intensity i emits the quantity of light $4\pi i$. The surface is always to be supposed perfectly white. In the case of actual surfaces (which are never quite white—that is, whose whiteness, a , is always less than 1), the formula has simply to be multiplied by a .

This being premised, the following principles hold good.

(1) A luminous point of intensity i at a distance r imparts to an element of surface, whose normal makes the angle α with the line joining the element and the source of light, a luminosity

$$H = \frac{i}{r^2} \cos \alpha.$$

(2) If a, b, c are the rectangular coordinates of the luminous point, x, y, z those of the element of the surface, and if this is at right angles to the axis of X , its brightness is

$$H_x = \frac{i}{r^3} \frac{a-x}{r} = \frac{d\left(\frac{i}{r}\right)}{dx}.$$

(3) If the element of surface in the point x, y, z , at right angles to the X axis, is simultaneously illuminated by several sources of light, all of which are on the same side of the element, its brightness is

$$H_x = \frac{d\Sigma i}{dx},$$

or, if $\Sigma \frac{i}{r} = V$,

$$H_x = \frac{dV}{dx}.$$

The brightness H of any element (presupposing that all sources of light are on the same side of the element) is

$$H = \frac{dU}{dn},$$

meaning by dn the element of the perpendicular to the element of surface.

From these principles the following result is obtained:—

Given any number of luminous points, the brightness of any surface illuminated by them at any given place is obtained by regarding the luminous points as material, and their masses as proportional to the intensities of the sources of light, next seeking the potential function of these masses, and then forming the differential quotients of the normals according to the element of surface.

This principle holds, of course, only under the above-made limi-

tation. If the brightness at any place the tangent-plane of which prolonged passes between the luminous points is to be sought, it is sufficient to form the potential for those points which lie upon one side of this plane.

Hence the principle is quite general for surfaces which surround all the sources of light in such a manner that from each of their points all the luminous points can be seen.

(4) Given a number of luminous points, all of which are within an enclosed surface, if H is the brightness of an element of surface $d\sigma$, then

$$\int \frac{dV}{dn} d\sigma = \int H d\sigma = 4\pi \Sigma i + 2\pi \Sigma i' + \Sigma \omega i''$$

when the integral is extended over the whole surface. Here i is the intensity of a source of light which is within, i' that of a source of light which lies in the surface at a place where it is uniformly curved; i'' is the intensity of a luminous point at an angle or edge of the surface; while ω is the piece of the surface which is cut out by the tangent cone laid through such a point from the surface of the sphere of radius unity described about the point.

This is the analogue of one of the fundamental principles of attraction. But while in that case its accuracy can only be recognized after proof, from the point of view of photometry it is at once clear. $\int H d\sigma$ is nothing more than the whole quantity of light falling upon the inner side of the surface; and that this is represented by the expression on the right-hand side requires no further demonstration.

Given now a number of luminous points, all of which are external to an enclosed surface, the two sides of which are affected by opposite signs, if then the brightness at all points of the surface be sought, assuming that the parts causing the shadow are withdrawn from before the parts shaded by the surface itself, then

$$\int H d\sigma = 0$$

when the integral is extended over the entire surface; and the brightness H gives the sign of the illuminated side of the surface.

In other words, after removing the shading parts, as much light falls upon the originally shaded places as formerly fell upon the shading parts.

(5) If V is the potential of acting masses, $V=C$ is the equation of a surface upon which the total force is everywhere at right angles.

In like manner in photometry, if $V = \Sigma \frac{i}{r}$, then $V=C$ is a surface which has the property that each of its elements is more brightly illuminated than any other element laid through the same point*.

Such a surface may therefore be conveniently called a *surface of greatest brightness*, and corresponds to a *level-surface*.

(6) If a series of surfaces of greatest brightness are laid in space which correspond to the values $V=A$, $V=A+dA$ to $V=A+n dA$, the brightness with which any element of a surface laid through a point of such a surface is illuminated is inversely proportional to the

* Of course always with the limitation mentioned under (3).

length which is cut off on the normal to the surface by the adjacent level-surface.

(7) If on a surface of greatest brightness which encloses the whole of the sources of light electricity is so distributed that its density is everywhere proportional to the brightness prevalent there, the electricity on this surface would be in equilibrium if it were the surface of a conductor.—Poggendorff's *Annalen*, No. 9, 1870.

Münich, April 1870.

A SOLAR FOG-BOW. BY R. C. CARRINGTON.

On the morning of November 27, at five minutes to ten, I saw a curious bow formed in a mist which was just rising; the sun was shining brightly; and on the principle of our motto, "*Quicquid nitet notandum*," I took immediate note of it. It was the smallest bow I have seen, not more than twenty-five paces from where I stood, and it measured thirty-five paces from bow to bow. I stepped a yard at a pace very nearly. The colour was white, like a lunar bow. But what was more remarkable was the appearance of a centre in which I could see the reflection of my head, which moved as I moved. I remember having seen a centre before in a rainbow, which I witnessed near Cader Idris, in Wales, in July 1847, in a splendid bow of two arches, but which I never mentioned before, having never heard of the like.—*Monthly Notices of the Astronomical Society*, January 13, 1871.

ON THE REFRACTIVE PHENOMENA OF AN ALCOHOLIC SOLUTION OF FUCHSIN. EXTRACT FROM A LETTER TO PROF. POGGENDORFF BY M. C. CHRISTIANSEN.

Copenhagen, Nov. 1870.

Permit me to inform you that I have been for some time occupied with investigations on the refractive power of red concentrated aniline (Fuchsine), and have obtained very curious results. Reserving for a future communication further details, I give here the ratios of refraction for an alcoholic solution containing 18·8 per cent. of aniline :—

Fraunhofer's lines.	Refraction-ratio.
B	1·450
C	1·502
D	1·561
F	1·312
G	1·285
H	1·312

The refraction-ratio increases from B to D and a little beyond, sinks then very rapidly to G, and then again increases.

This is readily observed by filling a very acute prism with the liquid; if a luminous slit be looked at through it, the colours are seen in the following order,—violet, red, yellow, in which the deviation of the latter is the greatest. The consequences are most easily and beautifully seen when the hypotenuse of a rectangular prism containing the solution is illuminated and the reflected light observed. Then, instead of the colours at the limit of total reflection, coloured light, rose-red, violet, blue, green, are observed under all incidences.—Poggendorff's *Annalen*, No. 11, 1870.

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LONDON, EDINBURGH, AND DUBLIN
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

[FOURTH SERIES.]

APRIL 1871.

XXXIII. *On the Capillary Phenomena of the common Surface of two Liquids.* By G. QUINCKE*.

I. *Flat drops or bubbles in various liquids.*

CONSIDERATIONS similar to those which have been applied to the free surface of a liquid (that is to say, bounded by space or by air) are applicable to the common surface of two liquids.

In the following memoir I shall use the same notation as in my former communications on Capillary Phenomena†, and shall distinguish the magnitudes which relate to a point P_1 or P_2 of the free surface of the liquid 1 or 2 by means of the suffix 1 or 2, the magnitudes which relate to a point P_{12} of the common surface of two liquids 1 and 2 by means of the double suffix 1 2.

Let R and R' denote the smallest and greatest radius of curvature of the common surface of two liquids 1 and 2 at the point P_{12} , then considerations analogous to those concerning free liquid surfaces show that in the direction of the surface-perpendiculars to the concave side of the surface at the point P_{12} a pressure is exerted

$$p_{12} = K_{12} + \frac{H_{12}}{2} \left(\frac{1}{R} + \frac{1}{R'} \right). \quad . \quad . \quad . \quad (1)$$

* Translated from Poggendorff's *Annalen*, vol. cxxxix. part. 1. having been communicated in abstract to the *Königl. Ges. d. Wissenschaften zu Göttingen* in October 1869.

† Pogg. *Ann.* vol. cv. pp. 1-48 (1858); vol. cxxxiv. pp. 356-367 (1868); vol. cxxxv. pp. 621-646; vol. cxxxviii. pp. 141-155 (1869).

The magnitudes K_{12} and H_{12} do not depend solely on the reciprocal action of two particles of the same liquid on one another, but also on the action which the particles of liquid 1 exert at a very small distance on the particles of liquid 2, and conversely.

It may be seen from the form of equation (1) that the common surface of two liquids, like the free surface of one liquid, will be as small as possible—that at the common surface of two liquids there will be a certain tension, as in a stretched membrane, which is the same at all points and is measured by the constant $\frac{H_{12}}{2}$ or α_{12} , which will be adopted instead of it in what follows.

The angle ω_{12} , at which the last element of the common surface of two liquids 1 and 2 cuts a side of the containing vessel, will only depend on the nature of the two liquids and of the side, and will be independent of the form of the common surface of the liquids and the form of the side.

The magnitude K_{12} , the perpendicular pressure on the plane common surface of two liquids 1 and 2, can just as little as the perpendicular pressure K_1 or K_2 on the free plane surface of the liquid 1 or 2 be determined when there subsists the relation

$$K_1 - K_2 = K_{12} = -K_{21}.$$

The weight on a unit of length of the line of section of a solid vertical side and the common surface of two liquids is

$$G_{12} = \frac{H_{12}}{2} \cos \omega_{12} = \alpha_{12} \cos \omega_{12}, \quad . \quad . \quad . \quad (2)$$

a constant magnitude, which only depends on the nature of the two liquids and the side.

α_{12} is the tension, measured in milligrammes, which is exerted on a space of the common surface of the breadth of a millimetre.

The propositions advanced in the foregoing, like those which hold good for free liquid surfaces, may either be deduced from the assumption of molecular forces, after the manner of Laplace's views, or the assumption of a surface-tension, after the manner of the views of Thomas Young.

2. The phenomena at the common limit of two liquids, apart from the well-known experiments of Plateau* on the figures of equilibrium of drops of oil on aqueous alcohol of the same specific gravity, have been but little investigated.

Gay-Lussac† observed the depression of mercury in a capil-

* "Recherches expérimentales et théoriques sur les figures d'équilibre d'une masse liquide sans pesanteur," Series I. to XI. *Mém. de l'Acad. de Brux.* vols. xvi. to xxxvii. (1842-68). 4to.

† Laplace, *Œuvres*, vol. iv. pp. 496 & 524.

lary tube when water, or alcohol containing water, was poured on it. Bède* observed the height to which water or sulphuric acid ascended in capillary tubes when, above one of these liquids, petroleum was poured into the tube, and that to which chloroform ascended when water was poured over the latter.

Thomas Young† found that a small quantity of oil poured on the surface of water in a capillary tube diminished the total height.

In these experiments, however, we have two unknown magnitudes:—the superficial cohesion, α_{12} , of the common surface of the two liquids; and the marginal angle ω_{12} , which, as I shall afterwards show (§ 10), is not always either 0° or 180° . The product $\alpha_{12} \cos \omega_{12}$ only is determined by observing the height.

I myself‡ have measured the height of drops of mercury in water and hydrochloric acid, without ever being able to draw further conclusions regarding the cohesion of the common surface of those liquids.

Guthrie§, lastly, dropped into different liquids other liquids, and measured the magnitude of the drops.

Even in the few cases on which experiments are given, theory has not been proved to agree with experience—partly because, beside the propositions mentioned above, and $\omega_{12}=0^\circ$ or 180° , relations also were assumed between the magnitudes α_1 , α_2 , and α_{12} , which, as I shall presently show, do not altogether agree with experiment.

The error of the usually assumed relation $\alpha_1 - \alpha_2 = \alpha_{12}$ suggests, moreover, the simple idea that for two liquids which mix in all proportions the capillary constant of the common surface α_{12} is $=0$, whilst for water and absolute alcohol it should be $8 - 2 \cdot 5 = 5 \cdot 5$ milligrammes.

Hence the proposition adduced by Poisson||, that the weight raised in a capillary tube will depend only on the lower liquid, is not in agreement with experience.

Thomas Young supposed the capillary constant of the common surface of two liquids to be proportional to the difference¶ or to the square** of the difference of their densities. Even this does not agree with the experiments which will be presently described (§ 10).

* *Mém. Cour. Brux.* vol. xxx. p. 187 (1860).

† *Encyc. Brit.* "Cohesion," Sect. II. (1816). Young's Works, by Peacock, vol. i. p. 463 (1855).

‡ *Pogg. Ann.* vol. cv. p. 38 (1858).

§ *Proc. Roy. Soc.* vol. xiii. p. 444, vol. xiv. p. 22 (1865). In abstract, *Pogg. Ann.* vol. cxxxi. p. 141 (1867).

|| *Nouvelle théorie de l'action capillaire*, p. 142.

¶ Young's Works, vol. i. p. 435 (1855). *Encycl. Brit.* "Cohesion," Sect. II. (1816).

** *Ibid.* p. 463.

A numerical value for the magnitude α_{12} , the surface-tension of the common surface of two liquids, is, to my knowledge, nowhere given.

3. If we denote by z the vertical distance of a point P_{12} , of the common surface of two liquids of specific gravity σ_1 and σ_2 , from the horizontal part of the common surface of the liquids, and let the positive axis of z coincide with the direction of gravity, and then from equation (1), and the hydrostatical proposition that in a horizontal plane in the interior of the same liquid there must everywhere be the same pressure, the equation follows,

$$(\sigma_1 - \sigma_2)z = \alpha_{12} \left(\frac{1}{R} + \frac{1}{R'} \right) \dots \dots \dots (3)$$

This equation would relate, for example, to the case where a spread drop of mercury in water is poured on a horizontal base. If the diameter of this drop is large, or if it be poured into a tray-shaped channel, then R' is very large, $\frac{1}{R'}$ to be neglected as compared with $\frac{1}{R}$, and equation (3) becomes

$$z = \frac{\alpha_{12}}{\sigma_1 - \sigma_2} \frac{1}{R} = \frac{a_{12}^2}{2} \cdot \frac{\frac{d^2 z}{dx^2}}{\left[1 + \left(\frac{dz}{dx} \right)^2 \right]^{\frac{3}{2}}}, \dots \dots \dots (4)$$

in which the specific cohesion of the common surface of the liquids,

$$a_{12}^2 = \frac{2\alpha_{12}}{\sigma_1 - \sigma_2}, \dots \dots \dots (5)$$

is introduced instead of the capillary-constant or surface-tension α_{12} .

The integration of equation (4) gives

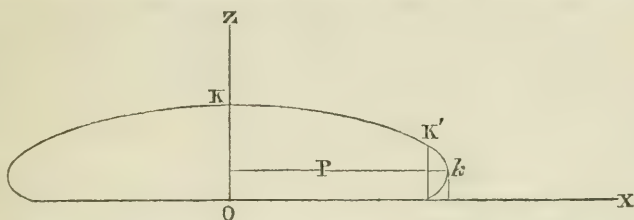
$$\left. \begin{aligned} \frac{z^2}{a_{12}^2} &= \text{const.} - \frac{1}{\sqrt{1 + \left(\frac{dz}{dx} \right)^2}}, \\ \frac{z^2}{a_{12}^2} &= 1 - \cos o_{12}, \end{aligned} \right\} \dots \dots \dots (6)$$

in which o_{12} is the angle which the element of the curve of the meridional section of the surface makes with the horizontal x axis. For the horizontal upper surface of the drop, z and o_{12} simultaneously equal zero.

For a vertical element of the curve of the meridional section

$$\left. \begin{aligned} z &= \bar{z}, \quad o_{12} = 90^\circ, \\ \frac{z^2}{a_{12}^2} &= 1, \quad a_{12} = \bar{z}; \end{aligned} \right\} \dots \dots \dots (7)$$

that is to say, *the vertical distance of the horizontal from the vertical element of the meridional curve* (of the point K from the point *k* in the accompanying figure, which in future will always



be denoted by $K - k$), measured in millimetres, gives, when squared, the specific cohesion of the common surface of the two liquids—and if multiplied by half the difference of the specific gravity, the capillary constant a_{12} or the tension of the surface of the common limit of the two liquids.

Equation (7) also holds good, and strictly so, when a liquid rests at an angle of 180° against vertical plane sides—for example, mercury against a vertical glass plane wetted with alcohol or water.

If the drop rests on a horizontal base, and if the vertical distance of the summit of the drop K and of the vertical meridional element k from the horizontal base be denoted by K and k respectively, then

[illegible]

$$K = z_{\theta_{12} = \omega_{12}} = a_{12} \sqrt{1 - \cos \omega_{12}}, \quad . \quad . \quad . \quad (9)$$

in which ω_{12} is the angle at which the last element of the surface of the drop cuts the horizontal base. By combining equations (8) and (9) the values of a_{12} and $\cos \omega_{12}$ may be obtained.

If the horizontal base is wetted by liquid 2, which frequently happens, then

$$\left. \begin{array}{l} \omega_{12}=180^\circ, \\ K=a_{12}\sqrt{2}. \end{array} \right\} \cdot \cdot \cdot \cdot \cdot \cdot (10)$$

$$K = a_{12}\sqrt{2}.$$

Accordingly as $\sigma_1 >$ or $< \sigma_2$ will z , K , and k be positive or negative. A drop of oil in water under a horizontal glass plate wetted with water has the same form as a drop of water in oil on a horizontal base wetted with oil, and so forth.

It is obvious that equations (2) to (9) are transformed into those for free liquid surfaces when σ_2 or σ_1 is put equal to zero. In the first case the drops are exposed to the air or a vacuum; in the second case they are air-bubbles which rest against either a horizontal or a slightly curved side. Of course, in the second case, the magnitudes K , k , and γ are always negative.

An air-bubble in water under a moistened horizontal plane glass has the same form as a drop of water on a horizontal base which it does not wet at all—for example, on a woollen cloth or a glass plate sprinkled with lycopodium-powder.

If the vertical distance $-(K-k)$ of the lowest point of an air-bubble from the vertical meridional element of its surface be measured, a_2 is obtained; if the distance $-K$ of the extremity of the air-bubble from the horizontal glass plate perfectly wetted with liquid 2, then $a_2\sqrt{2}$ is obtained.

This gives a method by which the capillary-constants of transparent liquids may be determined which has this great advantage—that the liquid in question only comes into contact with air, and the free liquid-surface is protected as much as possible from impurities, which even in small quantities, as I shall afterwards show, may cause the tension of the surface or capillary-constant to appear considerably too small.

4. In order to compare the capillary-constants of liquids examined according to other methods with those derived from the observation of capillary heights h , the following method was adopted.

By means of a glass-blower's lamp threads of suitable thickness were drawn out of a thicker glass tube purified as much as possible; the upper ends were melted together, and passed through two caoutchouc rings, by which the capillary tube which was formed by the threads was held fast to a strip of plate glass 10 millims. broad and from 100 to 300 millims. in length. A scale of millimetres was etched on the strip of plate glass, which simultaneously with the lower end of the capillary tube was immersed in the liquid under examination. The height h to which the latter rose above the horizontal level of the liquid after the closed top of the capillary tube was cut off, was read on the scale by means of a horizontal telescope; the capillary tube was cut with a glass knife at the place of the meniscus fluid surface, and the greatest and least diameters of the sectional surface were determined by means of a microscope with an eye-glass micrometer. The latter had a scale of 100 divisions, of which tenths could be estimated. One division of the scale corresponded to a magnitude of from 0.007 to 0.001 millim., according to the magnifying-power used. The means of the estimated values of the diameters of the tubes are given in the following Tables under 2r. The last column contains the product

$$hr = a^2 \cos \omega. \quad . \quad . \quad . \quad . \quad . \quad . \quad (11)$$

I have neglected to apply a correction to the observed heights on account of the meniscus, as other unavoidable sources of error have a much greater influence than this correction.

As a matter of precaution, the meniscus was brought to a well-moistened place of the capillary tube by inclining or raising the latter in the caoutchouc rings.

By the methods described, glass tubes were obtained which were always easily wetted by the liquids applied, so that the angle ω could be put equal to zero, and hr gave directly the specific cohesion a^2 , from which, by multiplication with half the specific gravity $\frac{\sigma}{2}$, the capillary-constant α was obtained in milligrammes.

The specific gravities were determined with an hydrometer suspended by a thin platinum wire and a delicate balance.

TABLE I.—Heights in Capillary Tubes.

Water.				Hyposulphite of soda.			
No.	2r.	h.	hr.	No.	2r.	h.	hr.
	millim.	millims.	sq. mil.		millim.	millims.	sq. mil.
1.	0.252	115.2	14.19	1.	0.444	60.6	13.45
2.	0.287	101.7	14.57	2.	0.533	50.8	13.55
3.	0.356	81.2	14.44	3.	0.588	46.7	13.74
4.	0.529	55.7	14.74			Mean.....	13.58
5.	0.840	34.3	14.41				
		Mean.....	14.47				
Bisulphide of carbon.				Olive-oil.			
1.	0.305	34.4	5.240	1.	0.268	53	7.107
2.	0.344	30.7	5.284	2.	0.387	35.4	7.05
3.	0.364	29	5.290	3.	0.623	23.5	7.321
		Mean.....	5.273			Mean.....	7.159
Oil of turpentine.				Chloroform.			
1.	0.094	164	6.243	1.	0.079	95	3.742
2.	0.357	35.1	6.258	2.	0.280	26.3	3.683
3.	0.514	24.5	6.295	3.	0.334	21.9	3.662
4.	0.623	19.7	6.139	4.	0.838	8.6	3.606
		Mean.....	6.234			Mean.....	3.673
Petroleum.				Alcohol.			
1.	0.211	60.1	6.348	1.	0.086	132.9	5.720
2.	0.235	55.1	6.481	2.	0.331	33.6	5.562
3.	0.350	36.5	6.393	3.	0.606	18.8	5.696
4.	0.364	35.8	6.519			Mean.....	5.659
		Mean.....	6.434				

The means of these numbers, together with the values of a and α , as also the specific gravities σ and observed temperatures, are collected in the following Table:—

TABLE II.—Capillary Constants calculated from the heights in the Capillary Tubes.

No.	Substance.	α .	α^2 .	a .	Temp.	σ .
		mgrams.	sq. mil.	millims.		
1.	Hyposulphite of soda	7.636	13.58	3.684	21.9	1.1248
2.	Water	7.235	14.47	3.804	16.2	1.
3.	Bisulphide of carbon.	3.343	5.273	2.296	18	1.2687
4.	Olive-oil	3.271	7.159	2.675	22	0.9136
5.	Oil of turpentine	2.765	6.234	2.497	21.7	0.8867
6.	Chloroform	2.733	3.673	1.916	16.6	1.4878
7.	Petroleum	2.566	6.434	2.536	22.3	0.7977
8.	Alcohol	2.237	5.659	2.379	21.8	0.7906

5. Some rectangular troughs, 25 millims. in height and from 50 to 70 millims. in length and breadth, were made of plates of very pure plate glass stuck together with sealing-wax. The glass trough was filled with the liquid to be examined and placed on a horizontal glass plate before a cathetometer, whose horizontal microscope or telescope was provided with cross-wires or a glass divided as a micrometer, and whose horizontal and vertical displacement could be measured to 0.001 of a millimetre. The horizontal glass plate was cemented on a wooden stand provided with three adjusting-screws. A narrower plate-glass cover was laid on the glass trough filled with the liquid under examination, and was placed horizontally by means of a level. With a clean glass thread just drawn out before the lamp, of from 0.5 to 2 millims. diameter, it is easy, after a little practice, to blow an air-bubble of suitable magnitude (about 20 or 30 millims. in diameter) under the glass cover. The glass trough was so shifted that the contour of the air-bubble, which was suitably illuminated from behind, appeared sharp in the range of the microscope, and then the heights K and k were measured as quickly as possible. The objective of the microscope had a diameter of 16 millims., and was about 120 millims. from the object. The whole arrangement was similar to that which I had before used for the measurement of drops of mercury*.

The glass trough and plates must be as clean as possible, and well wetted with the liquid under examination.

In the following Tables are collected the observations on a series of air-bubbles in an aqueous solution of hyposulphite of soda, distilled water, bisulphide of carbon, olive-oil, oil of turpentine, petroleum, and absolute alcohol.

* Compare Pogg. *Ann.* vol. cv. p. 15, and pl. i. figs. 4, 5, 11, 12 (1858).

For bisulphide of carbon, oil of turpentine, and petroleum, the plates forming the trough were fastened together with common glue; for the experiments on absolute alcohol, with pure paraffine. For the sake of simplicity the negative signs are omitted before K , k , and $K-k$. $2r$ gives the greatest diameter of the drop, or the horizontal distance of two opposite vertical meridian-elements k . When, in the column for $2r$, two commas denote that the diameter of the air-bubble is the same as that of the one above, both observations relate to the same air-bubble, but the second was made a longer time after the rising of the air-bubble than the first. The value of the capillarity-constant α in the last column is calculated by means of equations (4) and (7) (§ 3). The rest of the arrangement of the Tables speaks for itself.

TABLE III.—Flat Air-bubbles in a solution of Hyposulphite of Soda. $\sigma = 1.1248$.

No.	$2r$.	K .	k .	$K-k$.	$K \sqrt{\frac{1}{2}}$.	α .
	millims.	millims.	millims.	millims.	millims.	mgrms.
1.	20	5.970	1.302	3.768	3.585	7.984
2.	"	5.015	1.189	3.826	3.546	8.233
3.	28.2	5.358	1.620	3.738	3.788	7.856
4.	"	5.323	1.661	3.662	3.763	7.541
					Mean.....	7.903

Flat Air-bubbles in Distilled Water.

$\sigma = 1$. Temp. = 25° C.

1.	18	5.628	1.516	4.112	3.978	8.455
2.	27.6	5.552	1.483	4.069	3.926	8.280
3.	21	5.509	1.537	3.972	3.895	7.905
4.	21.9	5.255	1.212	4.043	3.716	8.170
5.	20	5.082	1.071	4.011	3.594	8.040
6.	20.8	5.315	1.090	4.225	3.758	8.920
7.	30.7	5.612	1.612	4.000	3.969	8.000
					Mean.....	8.253

Flat Air-bubbles in Bisulphide of Carbon.

$\sigma = 1.2687$. Temp. = 25° C.

1.	(25)	3.071	0.891	2.180	2.171	3.015
2.	27	3.081	0.851	2.230	2.179	3.157
3.	(35)	3.121	0.722	2.399	2.207	3.651
					Mean.....	3.274

Flat Air-bubbles in Olive-oil.

 $\sigma=0.9136$. Temp. = $25^{\circ}8$ C.

No.	2r.	K.	k.	K-k.	$K\sqrt{\frac{1}{2}}$.	α .
	millims.	millims.	millim.	millims.	millims.	mgrms.
1.	30	4.027	1.026	3.001	2.848	4.113
2.	34	3.980	1.121	2.859	2.815	3.735
3.	"	4.031	1.146	2.885	2.850	3.804
4.	31.2	3.900	0.997	2.903	2.758	3.850
5.	"	3.919	1.105	2.814	2.771	3.617
6.	29.7	3.978	1.227	2.751	2.813	3.457
7.	"	4.052	1.191	2.861	2.865	3.741
					Mean	3.760

Flat Air-bubbles in Oil of Turpentine.

 $\sigma=0.8867$. Temp. = $25^{\circ}1$ C.

1.	?	3.512	0.958	2.554	2.483	2.892
2.	"	3.543	1.012	2.531	2.505	2.841
3.	26.8	3.402	0.737	2.665	2.406	3.149
4.	"	3.389	0.730	2.659	2.396	3.134
5.	?	3.509	0.889	2.620	2.481	3.044
6.	"	3.500	0.930	2.570	2.475	2.928
7.	?	3.580	0.901	2.679	2.531	3.181
8.	?	3.569	0.927	2.642	2.524	3.094
					Mean.....	3.033

Flat Air-bubbles in Petroleum.

 $\sigma=0.7977$. Temp. = $24^{\circ}2$ C.

1.	29.5	3.788	0.950	2.838	2.679	3.212
2.	(30)	3.869	1.009	2.860	2.736	3.263
3.	(30)	3.818	0.960	2.858	2.701	3.258
					Mean.....	3.233

Flat Air-bubbles in absolute Alcohol.

 $\sigma=0.7906$. Temp. = $25^{\circ}3$ C.

1.	31.4	3.402	0.870	2.532	2.406	2.533
2.	"	3.570	1.010	2.560	2.524	2.591
3.	28.3	3.591	0.980	2.611	2.539	2.695
4.	"	3.528	0.980	2.548	2.496	2.566
5.	28.2	3.540	1.001	2.539	2.503	2.548
6.	"	3.523	0.963	2.560	2.491	2.590
7.	28.8	3.586	1.013	2.573	2.536	2.616
8.	"	3.578	0.987	2.591	2.530	2.655
					Mean.....	2.599

If the mean values of α of the single observations on air-

bubbles in Table III. be compared with the values of the same constant of Table II. derived from the capillary heights, the former will be found greater than the latter. For the sake of easy inspection these values are placed side by side in the following Table, IV.

This difference partly results from the assumption that the air-bubble has a plane top and an infinitely large radius R' at all points of its surface. Now $R'=r$ for the vertical meridian-elements, and is less than at the top of the air-bubble, where the principal radii of the curves are equal and very large. Thus the magnitude $\frac{\alpha}{R'}$ for the vertical meridian-element may be greater than the magnitude $\frac{2\alpha}{R'}$ for the surface-element of the top of the air-bubble, or the capillary pressure in the vertical meridian-element of the air-bubble greater than was assumed in the approximative calculation in § 2. In this case $K-k$ must be found greater than the actual value of the constant a .

After the production of the capillary surface the constant α gradually diminishes with the time, and α must be found just so much the greater the quicker it can be observed. Generally a drop gains its equilibrium much quicker than a column of liquid which rises in a capillary tube; and for this reason the value of the capillary constant must also be found greater in a drop or an air-bubble than from the heights in tubes. For this reason also the diameters of the capillary tube should not be chosen too small.

If the angle ω were really 180° , then would

$$K\sqrt{\frac{1}{2}}=K-k=a.$$

But this only happens in the fewest cases, as Table III. shows.

If from the separate Tables the mean of $K\sqrt{\frac{1}{2}}$ be taken and the following expression formed,

$$\bar{\alpha} = \frac{K^2}{2} \cdot \frac{\sigma}{2}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (12)$$

then $\bar{\alpha}$ will always be $< \alpha$, as Table IV. shows.

But according to equations (4) and (8), § 2,

$$\bar{\alpha} = a^2 \cdot \frac{\sigma}{2} \cdot \frac{1 - \cos \omega}{2} = \alpha \cdot \sin^2 \frac{\omega}{2},$$

$$\theta = 180^\circ - \omega = 180^\circ - 2 \arcsin \left(\sin = \sqrt{\frac{\bar{\alpha}}{\alpha}} \right). \quad . \quad (13)$$

The last column but one of Table IV. gives the values of the acute angle θ calculated from equation (13), the last column the values of $\cos \theta$ reckoned from columns 4 and 6.

TABLE IV.—Capillary constants α of the free surface of Liquids at the mean Temperature.

No.	Substance.	Capillary heights in tubes,	Drops.		θ .	$\alpha \cos \theta$.
		$a = \sqrt{hr}$.	$a = K - k$.	$a = K \sqrt{\frac{1}{2}}$.		
		α .	α .	$\bar{\alpha}$.		
		mgrms.	mgrms.	mgrms.		mgrms.
1.	Mercury	55.030	44.600	51° 8'	
2.	Hyposulphite of soda.	7.636	7.903	7.580	23 20	7.256
3.	Water	7.235	8.253	7.850	25 32	7.449
4.	Bisulphide of carbon.	3.343	3.274	3.021	32 16	2.768
5.	Olive-oil	3.271	3.760	3.625	21 50	3.490
6.	Oil of turpentine ...	2.765	3.033	2.716	37 44	2.398
7.	Chloroform	2.733	3.120			
8.	Petroleum	2.566	3.233	2.918	36 20	2.604
9.	Alcohol	2.237	2.599	2.476	25 12	2.352

The value of α for chloroform, contained in this Table, was derived from the height \bar{z} on a moistened glass plane, where consequently the angle ω was made equal to 0. It was found that

$$\bar{z} = 2.114 \text{ millims.}, \quad 1.949 \text{ millim.}, \quad 2.078 \text{ millims.};$$

or the mean of

$$a = 2.047 \text{ millims.}, \quad a^2 = 4.191 \text{ sq. millims.}, \quad \alpha = 3.12 \text{ milligrms.}$$

The values for mercury are taken from the observations given in the following section.

Notwithstanding the inexactness of the value of the angle θ , which was only determined approximately, the numbers in the last column, which almost agree with those in the third column of Table IV., show how unjustifiable it is to assume that the angle of the capillary meniscus in glass tubes is equal to zero, and from the capillary heights to determine the capillary constants of the liquids in question by the help of this assumption.

The variations which exist in the numbers of the third and last columns for oil of turpentine and bisulphide of carbon may very likely be caused by a chemical change in these liquids, as between the measurements of the flattened air-bubbles and capillary heights of these liquids there was accidentally an interval of several weeks.

6. For flattened drops of mercury on clean glass plates in the air, measurements were made, in the shortest time possible after the formation of the same, similar to those made for flattened air-bubbles. Table V. gives the results. The values of K , k , and $K - k$ are, of course, positive.

TABLE V.—Flat drops of Mercury in Air.

$$\sigma = 13.5432. \quad \text{Temp.} = 20^{\circ} \text{ C.}$$

No.	2r.	K.	k.	K-k.	α .
	millims.	millims.	millim.	millims.	mgrms.
1.	28.9	3.463	0.529	2.934	58.30
2.	?	3.555	0.705	2.850	54.99
3.	(20)	3.722	0.851	2.871	55.84
4.	(20)	3.731	0.939	2.792	52.78
5.	(20)	3.630	0.857	2.773	52.07
6.	32.5	3.628	0.789	2.839	54.56
7.	34	3.649	0.788	2.861	55.46
8.	33	3.656	0.774	2.882	56.24
				Mean	55.03

The mean height K of the drops is 3.629 millims., from which, and by means of equations (12) and (13), it follows that the acute angle of mercury against glass, when $\alpha = 55.03$ milligrammes, is

$$\theta = 51^{\circ} 8'.$$

7. Drops of a liquid of specific gravity σ_1 , in another liquid of specific gravity σ_2 , behave similarly to drops of mercury in air when $\sigma_1 > \sigma_2$.

In this relation I examined bisulphide of carbon and chloroform in water which was placed in a trough whose sides were made of plate glass and cemented with sealing-wax. The drops of liquid introduced into the water did not come into contact with the sealing-wax. Without this precaution, α_{12} will be found less than in the following Tables, in which the observations are collected. The measurements were made as described in § 5. The notation is the same as there, except that K, k, and K-k are now positive magnitudes.

TABLE VI.—Flat drops of Bisulphide of Carbon in Water.

$$\sigma_1 = 1.2687, \quad \sigma_2 = 1, \quad \frac{\sigma_1 - \sigma_2}{2} = 0.1343.$$

No.	2r.	K.	k.	K-k.	$K\sqrt{\frac{\sigma_1}{\sigma_2}}$.	α .
	millims.	millims.	millims.	millims.	millims.	mgrms.
1.	25.5	7.878	2.368	5.510	5.571	4.069
2.	"	7.887	2.409	5.478	5.577	4.021
3.	"	7.536	2.146	5.390	5.328	3.893
4.	27.1	7.960	2.130	5.830	5.628	4.555
5.	"	8.091	2.283	5.808	5.723	4.520
6.	?	8.000	2.217	5.783	5.657	4.480
				Mean.....	4.256	

Flat drops of Chloroform in Water.

$$\sigma_1 = 1.4878, \quad \sigma_2 = 1, \quad \frac{\sigma_1 - \sigma_2}{2} = 0.2439.$$

No.	2r.	K.	k.	K-k.	K $\sqrt{\frac{1}{2}}$.	α .
	millims.	millims.	millims.	millims.	millims.	mgrms.
1.	28.9	4.891	1.341	3.550	3.458	3.073
2.	"	4.614	1.325	3.289	3.263	2.638
3.	32.1	5.170	1.383	3.787	3.656	3.497
4.	(36)	4.935	1.485	3.450	3.490	2.902
5.	"	4.825	1.353	3.472	3.411	2.941
					Mean.....	3.010

It may be observed with regard to this and the following experiments, that the liquids show the greatest cohesion at the moment they come together; they then intermingle*; consequently they become similar to one another in the neighbourhood of the limiting layer; and by these means the tension α_{12} of the common surface must diminish, which would certainly be 0 as soon as liquids 1 and 2 were the same. This decrease is evident from the experiments.

If bisulphide of carbon and water remain a long time in contact, an air-bubble is formed on the top of the drop of bisulphide of carbon, which gradually increases in size.

Analogous observations were made on drops of olive-oil, oil of turpentine, and petroleum, the production of which was similar to that of air-bubbles (compare § 5) under a horizontal glass plate in water. The observed values of K, k, and K-k are negative; for the sake of brevity, however, the negative signs are omitted in the following collection of observations.

* According to Dupré (*Théorie Mécanique de la Chaleur*, Paris 1869, Svo, p. 373), the diffusion of two liquids will always take place as soon as $\alpha_1 + \alpha_2 < 2F'$, where $2F' = \alpha_1 + \alpha_2 - \alpha_{12}$ (*loc. cit.* p. 370). According to these data, then, two liquids would diffuse as soon as α_{12} , the surface-tension of the common limiting surface, became negative; but according to my experience, not only do they diffuse for $\alpha_{12} = 0$, but also when α_{12} has a very appreciable, of course positive, value.

TABLE VII.—Flat drops of Olive-oil in Water.

$$\sigma_1 = 0.9136, \quad \sigma_2 = 1, \quad \frac{\sigma_2 - \sigma_1}{2} = 0.04318.$$

No.	2r.	K.	k.	K-k.	K- $\sqrt{\frac{1}{2}}$.	α .
	millims.	millims.	millims.	millims.	millims.	mgrms.
1.	32.9	7.006	2.120
2.	"	9.662	2.633	7.029	6.832	2.134
3.	45.2	10.052	2.772	7.280	7.108	2.289
4.	42.1	9.770	2.896	6.874	6.908	2.042
5.	"	9.677	2.838	6.839	6.842	2.020
6.	47.4	9.719	2.778	6.941	6.873	2.082
7.	"	9.592	2.807	6.785	6.782	1.988
					Mean.....	2.096

Flat drops of Oil of Turpentine in Water.

$$\sigma_1 = 0.8867, \quad \sigma_2 = 1, \quad \frac{\sigma_2 - \sigma_1}{2} = 0.05665.$$

						α_{12} .
1.	16.5	6.276	1.764	4.512	4.438	1.150
2.	"	6.120	1.760	4.360	4.328	1.074
3.	"	6.128	1.602	4.526	4.334	1.157
4.	18.7	6.368	1.479	4.889	4.503	1.350
5.	"	6.279	1.538	4.741	4.440	1.270
6.	"	6.295	1.685	4.610	4.451	1.200
7.	17.5	6.080	1.545	4.535	3.832	1.162
8.	12.9	5.620	1.350	4.270	3.974	1.030
9.	23	6.400	1.790	4.610	4.526	1.200
					Mean.....	1.177

Flat drops of Petroleum in Water.

$$\sigma_2 = 0.7977, \quad \sigma_1 = 1, \quad \frac{\sigma_2 - \sigma_1}{2} = 0.1012.$$

1.	21.6	7.735	2.155	5.580	5.470	3.147
2.	"	7.980	2.060	5.920	5.643	3.542
3.	26.1	8.312	1.650	6.662	5.878	4.382
4.	"	8.180	1.683	6.497	5.785	4.267
					Mean.....	3.834

It also follows from these observations that the liquids intermingle, and the capillary constant α_{12} becomes smaller the longer the liquids remain in contact.

The numbers in column K $\sqrt{\frac{1}{2}}$ are less than those in column K-k, from which it would follow that the angle θ_{12} was not 0. The values of this angle, calculated by means of equations (8) and (9), § 3, are given in the last column of Table X. § 10.

8. I have further measured the capillary constants of the common surface of mercury with other liquids.

The observations collected in Table VIII. were made on flat drops of mercury in an aqueous solution of hyposulphite of soda, water, and olive-oil, like those on flat drops of bisulphide of carbon or chloroform in water (§ 7). For the observations on flat drops of mercury and petroleum a trough was made of plates of plate glass cemented with common glue; for observations in absolute alcohol they were cemented with pure paraffin.

In order to measure the vertical distance of the horizontal from the vertical surface-element in flat drops of mercury on a horizontal glass plate in a liquid placed in a glass beaker, a flexible thick copper wire with a vertical thin sewing-needle at the end, was fastened to the cathetometer. The needle-point was then moved so as to touch the top of the drop or the horizontal glass plate (where the needle-point and its image must have touched one another), or was placed at an equal height with the vertical meridian-element of the drop. The latter adjustment, however, is difficult and inexact.

The determinations from Nos. 3 to 7, for mercury and bisulphide of carbon, were made in this manner: for Nos. 3 to 5 the values of a_{12} were obtained from $K - k$; for Nos. 6 and 7 from $K\sqrt{\frac{1}{2}}$, by which the marginal angle was put $= 180^\circ$.

Another part of the observations were conducted in the following manner. A horizontal glass tube 18.3 millims. in length and 25.8 millims. in diameter was closed at the ends by vertical glass disks, which were pressed by screws against the evenly cut thick wall of the glass tube. The latter was half filled with mercury through an opening in the upper part of the tube; and upon this the liquid was poured, for whose common limit with mercury the capillary constant was to be determined. The depression of the mercury at the vertical glass plane moistened by the specifically lighter liquid was measured with the cathetometer; and this was then, assuming the angle to be 180° , the constant denoted above (equation 6, § 3) by a_{12} . Observations 1 and 2 for bisulphide of carbon, 1 and 2 for petroleum, and 1 to 3 for chloroform and mercury were made in this manner.

By inclining the vessel and immediately observing the capillary depression on the vertical glass side, I endeavoured to make the marginal angle $= 180^\circ$. If the liquids are left long in contact with the glass side, then in general the acute angle $\theta_{12} > 0^\circ$, as follows, for example, from the observations on turpentine and mercury (see Table VIII.), where the angle θ_{12} was found, from the determinations of the height of the drop K , to be $47^\circ 2'$, and from the depression on a glass plate to be $11^\circ 54'$.

For the observations on flat drops of mercury in petroleum

and alcohol the values of $K\sqrt{\frac{1}{2}}$ are partly greater than the corresponding values of $K-k$; so that I then put

$$\theta_{12}=180^\circ-\omega_{12}=0,$$

and have taken the mean of the determinations

$$\alpha_{12}=(K-k)^2\frac{\sigma_1-\sigma_2}{2}, \quad \bar{\alpha}_{12}=\frac{K^2}{2}\frac{\sigma_1-\sigma_2}{2}. \quad (14)$$

TABLE VIII.—Flat drops of Mercury in aqueous solution of Hyposulphite of Soda.

$$\sigma_1=13\cdot543, \quad \sigma_2=1\cdot1248, \quad \frac{\sigma_1-\sigma_2}{2}=6\cdot209.$$

No.	2r.	K.	k.	K-k.	$K\sqrt{\frac{1}{2}}$.	α_{12} .
	millims.	millims.	millims.	millims.	millims.	mgrms.
1.	16·3	3·722	1·027	2·695	2·631	45·10
2.	16	3·659	1·052	2·607	2·587	42·19
3.	16·4	3·703	1·085	2·618	2·618	42·54
4.	27·3	3·937	1·101	2·836	2·784	49·94
5.	"	3·828	1·098	2·730	2·707	46·28
6.	"	3·699	1·019	2·680	2·616	44·59
Mean.....						45·107

Flat drops of Mercury in Water.

$$\sigma_1=13\cdot543, \quad \sigma_2=1, \quad \frac{\sigma_1-\sigma_2}{2}=6\cdot271.$$

1.	28·9	3·538	0·905	2·633	2·502	43·49
2.	"	3·572	0·890	2·682	2·525	45·10
3.	(30)	3·564	1·004	2·560	2·520	41·10
4.	(30)	3·643	1·038	2·605	2·576	42·56
5.	(30)	3·613	1·006	2·607	2·555	42·62
6.	32·5	3·581	0·958	2·623	2·532	43·15
7.	30	3·611	1·084	2·527	2·554	40·05
Mean.....						42·58

Flat drops of Mercury in Olive-oil.

$$\sigma_1=13\cdot543, \quad \sigma_2=0\cdot9136, \quad \frac{\sigma_1-\sigma_2}{2}=6\cdot315.$$

1.	?	2·988	0·677	2·311	2·113	33·71
2.	"	3·101	0·805	2·296	2·193	33·29
3.	29	3·177	0·815	2·362	2·247	35·23
4.	"	3·098	0·816	2·282	2·191	32·88
5.	?	3·194	0·825	2·369	2·258	35·42
6.	"	3·147	0·806	2·341	2·225	34·60
Mean.....						34·19

Flat drops of Mercury in Oil of Turpentine.

$$\sigma_1 = 13.543, \quad \sigma_2 = 0.8867, \quad \frac{\sigma_1 - \sigma_2}{2} = 6.328.$$

No.	2r.	K.	k.	K-k.	K $\sqrt{\frac{1}{2}}$.	α_{12} .	α_{12} .	θ_{12} .
	millims.	millims.	millim.	millims.	millims.	mgrms.		
1.	30	1.967	24.49		
2.	"	2.590	0.536	2.054	1.838	26.69	21.37	47 38
3.	"	2.611	0.567	2.044	1.846	26.42	21.57	46 26
4.	36.3	2.856	0.886	1.970	2.019	24.56	25.77	0
					Mean...	25.54		

Mercury and Oil of Turpentine.
Depression on a vertical glass plane.

No.	$z = a_{12} \sqrt{1 - \sin \theta_{12}}$.	$\alpha_{12}(1 - \sin \theta_{12})$.	θ_{12} .
	millim.	mgrms.	
1.	1.811	20.75	10 49
2.	1.787	20.20	12 4
3.	1.782	20.09	12 19
4.	1.787	20.20	12 3
		20.31	11 54

Flat drops of Mercury in Petroleum.

$$\sigma_1 = 13.543, \quad \sigma_2 = 0.7977, \quad \frac{\sigma_1 - \sigma_2}{2} = 6.373.$$

No.	2r.	K.	k.	K-k.	K $\sqrt{\frac{1}{2}}$.	α_{12} .	$\bar{\alpha}$.
	millims.	millims.	millim.	millims.	millims.	mgrms.	
1.	2.145*	29.32	
2.	2.118*	28.59	millims.
3.	38	3.016	1.006	2.010	2.133	25.74	28.99
4.	"	2.990	1.005	1.985	2.114	25.11	28.49
5.	(30)	3.026	0.806	2.220	2.140	31.42	29.18
6.	"	3.088	0.841	2.247	2.184	32.18	30.39
						28.94	

Flat drops of Mercury in Alcohol.

$$\sigma_1 = 13.543, \quad \sigma_2 = 0.7906, \quad \frac{\sigma_1 - \sigma_2}{2} = 6.376.$$

							$\bar{\alpha}_{12}$.
1.	(30)	2.443	38.05	
2.	"	3.698	1.124	2.574	2.615	42.21	43.60
3.	"	3.579	1.137	2.422	2.531	37.40	40.84
4.	25.6	3.562	1.065	2.497	2.519	39.75	40.42
5.	2.553	41.64	
6.	2.580	42.43	
					Mean	40.708	

Mercury and Bisulphide of Carbon.

$$\sigma_1 = 13.543, \quad \sigma_2 = 1.2678, \quad \frac{\sigma_1 - \sigma_2}{2} = 6.137.$$

No.	a_{12} .	α_{12} .
	millims.	mgrms.
1.	2.550	39.90
2.	2.580	40.85
3.	2.439	36.51
4.	2.543	39.68
5.	2.370	34.46
6.	2.491	38.09
7.	2.432	36.31
Mean		37.97

Mercury and Chloroform.

$$\sigma_1 = 13.543, \quad \sigma_2 = 1.4878, \quad \frac{\sigma_1 - \sigma_2}{2} = 6.027.$$

1.	2.618	41.29
2.	2.598	40.69
3.	2.586	40.29
Mean		40.71

9. The observations on flat drops of olive-oil in absolute alcohol were conducted similarly to those on drops of mercury in the same liquid, and gave the following results:—

TABLE IX.—Flat drops of Olive-oil in Alcohol.

$$\sigma_1 = 0.9136, \quad \sigma_2 = 0.7906, \quad \frac{\sigma_1 - \sigma_2}{2} = 0.0615.$$

No.	2r.	K.	k.	K - k.	$K\sqrt{\frac{1}{2}}$.	α .
	millims.	millims.	millim.	millim.	millim.	mgrm.
1.	27.1	2.149	0.289	1.860	1.519	0.213
2.	"	2.211	0.251	1.960	1.563	0.236
3.	28.1	2.314	0.339	1.975	1.636	0.240
4.	"	2.329	0.457	1.872	1.647	0.215
					Mean. ...	0.226

The capillary constant of the common surface of olive-oil and aqueous alcohol of nearly equal specific gravity is also interesting on account of Plateau's experiments.

I determined, in a glass which with the liquids had not been disturbed for a year or more, and in which the aqueous alcohol had obtained a greater specific gravity than the olive-oil, the

depression \bar{z} of the olive-oil at the vertical cylindrical surface of 90 millims. diameter. It was 14 millims. in a place where, according to reflected light, the marginal angle amounted to 180° , so that we have:—

Olive-oil and Aqueous Alcohol (Plateau's liquid).

$$\sigma_1 = 0.91599, \quad \sigma_2 = 0.92307, \quad \frac{\sigma_1 - \sigma_2}{2} = 0.003538.$$

$$\bar{z} = \alpha_{12}, \quad 14 \text{ millims.} \quad \alpha_{12}, \quad 0.6934 \text{ milligramme.}$$

10. The experimental results of the three foregoing sections are collected in Table X. For the sake of comparison the capillary constants α_1 and α_2 of the free surface of liquids 1 and 2 are placed next to the capillary constant α_{12} of the common surface of both liquids.

It is at once seen that the relation $\alpha_{12} = \alpha_1 - \alpha_2$ is not fulfilled (compare § 2), that α_{12} is always smaller than the greater capillary constant of the free surface of a liquid, but may also be less than the smaller of the constants α_1 or α_2 . The last, for example, is the case for oil of turpentine, olive-oil, or petroleum and water.

An influence of the specific gravity on the value of α_{12} is not perceptible from these observations, although the common surface of mercury with other liquids shows a greater capillary constant than the common surface of other, specifically lighter liquids, as was to be expected from the great specific weight of mercury and the great mass of mutually attracting particles.

The constant α_{12} shown for mercury and hydrochloric acid was calculated from earlier* observations of the height of the drop

$K = 3.514$ millims., by assuming $\frac{\sigma_1 - \sigma_2}{2} = 6.22$, and the angle $\omega_{12} = 180^\circ$.

The considerations of § 5, as well as equations (11) and (12), are directly applicable to the present observations as soon as the half difference of the specific gravity, or $\frac{\sigma_1 - \sigma_2}{2}$, is introduced instead of $\frac{\sigma}{2}$.

In the following Table the values of $\bar{\alpha}_{12}$ are calculated by the help of equation (12), the values of θ_{12} by equation (13) and the mean value α_{12} as it was found from observations of $K - k$.

For one series of substances the angle θ_{12} lies between θ_1 and θ_2 . For the other substances, petroleum and water excepted, it is less than both.

* Pogg. Ann. vol. cv. p. 39 (1858).

TABLE X.—Capillary constants of the common surface of two Liquids at a middle Temperature (20° C.).

No.	Substance.	σ_1 .	σ_2 .	$\pm \frac{\sigma_1 - \sigma_2}{2}$.	a_1 .	a_2 .	a_{12} .	θ_1 .	θ_2 .	θ_{12} .
1.	Mercury—hyposulphite of soda.....	13.543	1.1248	6.209	mgrams. 55.03	mgrams. 7.903	mgrams. 44.73	51 8	23 20	10 42
2.	Mercury—water	"	1	6.271	55.03	8.253	42.58	51 8	25 32	26 8
3.	Mercury—alcohol	"	0.7906	6.376	55.03	2.539	40.71	51 8	25 12	
4.	Mercury—chloroform	"	1.4878	6.027	55.03	3.120	40.71			
5.	Mercury—hydrochloric acid	"	(1.1)	6.22	55.03	(7.15)	38.41			
6.	Mercury—bisulphide of carbon	"	1.2687	6.137	55.03	3.274	37.97	51 8	32 16	
7.	Mercury—olive-oil	"	0.9136	6.315	55.03	3.760	34.19	51 8	21 50	47 2
8.	Mercury—petroleum	"	0.7977	6.373	55.03	3.233	28.94	51 8	36 20	
9.	Mercury—oil of turpentine	"	0.8867	6.328	55.03	3.030	25.54	51 8	37 44	47 2
10.	Bisulphide of carbon—water	1.2687	1	0.1343	3.274	8.253	4.183	32 16	25 32	13 8
11.	Petroleum—water	0.7977	"	0.1012	3.233	8.253	3.824	36 20	25 32	42 46
12.	Chloroform—water.....	1.4878	"	0.2439	3.120	8.253	3.010			
13.	Olive-oil—water	0.9136	"	0.0432	3.760	8.253	2.096	21 50	25 32	17
14.	Oil of turpentine—water	0.8867	"	0.0566	3.033	8.253	1.177	37 44	25 32	37 44
15.	Olive-oil—aqueous alcohol.....	0.9136	0.9231	0.0035	3.760	(2.907)	0.693			
16.	Olive-oil—alcohol.	0.9136	0.7906	0.0615	3.760	2.539	0.226	21 50	25 12	87 48

I must, however, observe that too much weight ought not to be laid on the approximative values found here for θ_{12} or θ_1 and θ_2 , since the values α_{12} and $\bar{\alpha}_{12}$, as I shall take another opportunity of arguing more closely, may also, from other grounds than those which have been accepted hitherto, prove to be different. The value given for θ_{12} with olive-oil and alcohol is too great, as a simple consideration of the drop shows, and a direct determination of θ_1 , θ_2 , and θ_{12} is always to be preferred to an indirect.

It may here be mentioned that for liquids which can be mixed in all proportions $\alpha_{12}=0$,—thus for *water and aqueous solution of hyposulphite of soda, water and alcohol, oil of turpentine and alcohol, oil of turpentine and olive-oil, oil of turpentine and bisulphide of carbon.*

If liquid 1 is identical with liquid 2, then α_{12} is of course equal to zero.

The capillary constant α_{12} measures the surface-tension of liquid 1 at the common limiting surface just as well as the tension of surface of liquid 2 in the neighbourhood of the common limit.

[To be continued.]

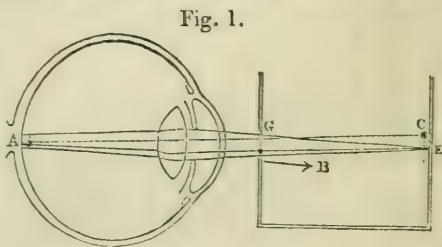
XXXIV. On an Optical Illusion. By JOSEPH LeCONTE*.

To Professor Tyndall, F.R.S.

University of California,
Oakland, California, Jan. 27, 1871.

DEAR SIR,
ONLY very recently my attention has been called to a very interesting and suggestive letter addressed to you by Mr. J. L. Tupper†, containing his explanation of an optical illusion. Permit me through the same medium to make some criticisms on Mr. Tupper's letter, which I hope will place the subject in a clearer light. I am sorry I did not see the letter sooner, as the criticisms would seem more appropriate if they had followed quickly the publication of the original.

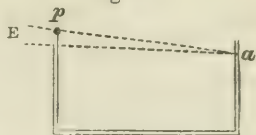
I reproduce here (fig. 1) Mr. Tupper's figure illustrating his experiment, only adding the dotted line to illustrate my own views. E is a radiant point of intense light, *e. g.* a pinhole through which light streams, G a larger hole in which is fixed a small pin, and through which a cone of light passes to the eye from E. Under these conditions the pin is seen inverted.



* Communicated by Prof. Tyndall. † Phil. Mag. vol. xxxix. p. 423.

A very simple and easy way of making the experiments, I have found, is as follows. Make a pinhole (*a*) in one end of a card-paper box (fig. 2). Through the

Fig. 2.




bottom at the other end thrust the pin (*p*) until the head stands a little above the margin of the box. Now with the eye at *E* work through the pinhole *a* at the sky or any strongly illuminated surface. The head of the pin will be seen in the pinhole inverted thus (!).

Mr. Tupper's explanation of this illusion is substantially as follows. In ordinary vision, as is well known, the retinal impression is made by conical bundles of rays from *radiant points* of the object converged to corresponding *focal points* in the image; while in this experiment "the rays of light pass from the radiant point *E* (fig. 1) and proceed to the lens, whence, after refraction, they slightly converge and end on the retina in a circle of light containing a dark space corresponding to a section of the pin." Now the impress in this case differs from that of natural vision in two respects: 1st, it is produced by *single rays* passing by the margin of the pin instead of *bundles of rays* converged to focal points; 2ndly, the impress in this case is *erect*, while in natural vision it is *inverted*. "Now it is manifest that two diametrically opposed positions of the pin on the sensitive retina will produce *perceptions* of diametrically opposite positions of the object." Pictures formed on the retina in the manner of this experiment, to distinguish them from ordinary retinal pictures, Mr. Tupper calls "*single-ray delineations*."

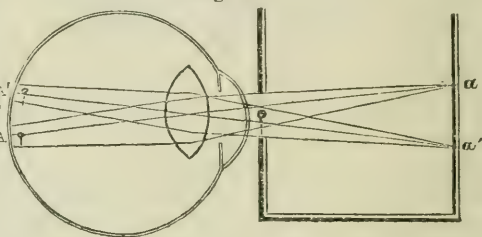
1. Now there is no doubt that this explanation is substantially correct; but it would have been much clearer if Mr. Tupper had distinctly expressed the fact that the retinal impression in his experiment is not an *image*, as in ordinary vision, but a *shadow*. Mr. Tupper seems to have perceived the distinction, but has not kept it clearly in his mind; and hence some confusion in his deductions. The extreme nearness of the pin is unfavourable for making an image and therefore for vision, but very favourable for making a strong shadow. The truth is we can hardly be said to *see* the pin at all. The shadow on the retina, by a well-known law, is projected outward into the field of vision and seen there inverted. The phenomenon is precisely similar to *musci volitantes*, which are also retinal shadows projected into the field of view and seen inverted. What proves incontestably that the retinal impress in Mr. Tupper's experiment is a shadow and not an image is, that, like shadows, it may be multiplied (as Mr. Tupper himself recognizes) to any extent by *multiplying*, not the pins, but the *radiant points*. In the experiment with the card-

paper box (fig. 2), this may be easily shown by making a number of pinholes at *a*. In each an inverted pinhead will be seen

thus: . But the *pinholes* are seen in their *true positions*,

though the *pins* are *inverted*. The reason is plain. The radiant cones from the several pinholes cross each other (fig. 3), as all radiant cones do, and their positions on the retina are therefore *inverted*; while the rays of each cone do not cross, as rays of radiant cones never do unless the retina is beyond the focal point; and the retinal shadows (*A*, *A'*) of the pin are therefore *erect*.

Fig. 3.



2. Mr. Tupper seems to think that his experiment overthrows the law of visible direction, and draws the arrow *B* (fig. 1) to show the direction in which, according to this law, the pinhead ought to be seen. But in this I think he is mistaken. The experiment confirms the law, as indeed does every phenomenon of vision. The law of visible direction is, that every point of the retinal impression is referred to the field of vision along a line passing through that point and through the optic centre. The visible direction is not represented by the line *AB*, but by the dotted line *AC* which I have added. This gives the pin *inverted* as we actually find it. How Mr. Tupper was led into this mistake I know not—unless he supposes that the ray *A*, followed back along its last course, gives the visible direction. In ordinary vision, it is true, the visible direction for each point is the *axial* ray of the cone followed back; but this is only because this ray passes through the optic centre. But Mr. Tupper says “the object is seen where it is not.” *Musci volitantes* are also seen where they are not, not only as to distance but also as to *direction*. Those *above* the optic axis are seen *below* the line of sight, and those *below* are seen *above*. The law of visible direction gives us the true position of objects seen by images, but not of objects seen by retinal shadows; but, properly understood, this is one of those apparent exceptions which prove the law.

3. The last paragraph of Mr. Tupper's letter I do not quite understand; but it certainly contains an erroneous statement. “It may be noticed,” he says, “that this analysis demonstrates the peculiar effect of what I call single-ray delineation on the retina,

which will happen not only when the radiant point is behind the object, showing it as a shadow, but also when an object, itself reflecting rays, is seen through a pinhole. The effect in both cases is faint vision and an *uninverted image on the retina*, which would result in both cases in *inverted* vision, save that in the latter there has been a *previous inversion* which sets things right." Now in this sentence there is an association, under the term single-ray delineation, of things which are entirely distinct. Nothing can be more certain than that vision through pinholes, in all essential respects, is precisely similar to ordinary vision. It is vision by retinal *image*, and not vision by retinal *shadow*. The image on the retina in this case, as in all cases of true vision, is formed by *cones* of rays from radiant points of the object concentrated to corresponding focal points in the image—the only difference being that the radiant cones in pinhole vision are very small, and the light therefore feeble. The axial rays of the radiant cones cross each other as in ordinary vision (though perhaps not exactly at the same point as in ordinary vision), and produce therefore an *inverted* image on the retina. In other words, a pinhole acts much as a very small pupil. The retinal impression formed by pinhole vision is therefore entirely different from that in Mr. Tupper's experiment. The one is a *true image*, the other a *shadow*; the one is *inverted*, the other *erect*.

What Mr. Tupper means by the last part of the sentence quoted above I cannot tell.

I fear I weary your patience with this long letter. Only one word more. Whether the seeing of objects in their true position be, in man, a *primary* or an *acquired* faculty may perhaps always be a vexed question. My own belief is that it is partly the one and partly the other—that a capacity is innate by virtue of which the very complex knowledge involved in vision is rapidly, *almost immediately* acquired. The actions of all animals are determined partly by experience and partly by instinct. But instinct itself is probably but an inherited capacity, improved by the experience of thousands of generations—a sort of *inherited experience*. In insects the wealth of inherited experience is great, and of individual experience is small; while in man the reverse is true. I think no one can doubt that a newborn chick or a newborn ruminant sees objects in their true position. Is it improbable, then, that in man also erect vision is partly the result of inherited experience, though confirmed and strengthened by individual experience—the former inherited through all human generations, or even perhaps (Darwinians would say) through all vertebrate generations?

Very truly and respectfully yours,

JOSEPH LECONTE.

XXXV. *Contributions to the Mineralogy of Nova Scotia.* By Professor How, D.C.L., University of King's College, Windsor, Nova Scotia.

[Continued from vol. xxxix. p. 280.]

VI. *Winkworthite, a New Mineral from the Gypsum of Hants County.*

SINCE I first showed the existence of borates in the gypsum and anhydrite of this district, in addition to the specimens collected by myself, some very good ones have been obtained by others. Mr. Selwyn, Director of the Geological Survey of the Dominion of Canada, to whom I had the pleasure of exhibiting my cabinet while on an official visit to this province during the past summer, was surprised at the considerable dimensions of some of the specimens I had secured, as he himself, on inspecting the gypsum quarries in company with Professor Hind, had only seen small nodules. My success was due, to some extent, to the good services of the quarrymen; for, having pointed out the different borates to some of these and to the manager of some of the quarries at Winkworth, the locality of the finest specimens of silicoborocalcite (Howlite of Dana), I was fortunate enough to bring about the preservation of a number of specimens fresh from the newly blasted rock. The quantity of gypsum raised at those of the Winkworth quarries now spoken of amounts on the average to about 22,000 tons a year; so that there is a good deal of material for examination. I may mention that the total export of "plaster" (*i. e.* gypsum and anhydrite), a few thousand tons only of the latter, however, being sent away, during the last two years from this county has been as follows:—

Plaster shipped from Hants County, Nova Scotia.

Port.	1869.	1870.	
Windsor . .	81,276	55,913	tons of 2240 lbs.
Hants port . .	3,860	7,567	"
Maitland . .	1,180	1,075	"
Cheverie . .	9,348	15,335	"
Walton . . .	4,760	5,640	"
Totals . .	100,424	85,530	"

During the past year, since the publication of the latest paper of this series (Phil. Mag. April 1870), very good specimens of the borates have been collected by quarrymen at Winkworth: some of these came directly to myself; for others I am indebted to my friend Mr. J. Brown, of this county, in whose cabinet I saw, last summer, the two largest specimens of crystalline Howlite yet observed, so far as I know. They consisted of rounded

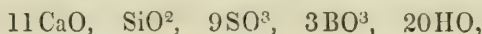
masses composed of white pearly scales : the largest was made up almost exclusively of Howlite ; there was a little overlying fibrous silky Ulexite, and a partial covering of the former by some grey earthy carbonated gypsum, of which the quantity was small ; the weight of the whole was about seven ounces avoirdupois : the second specimen was not much smaller ; the largest previously known to me weighed only about two ounces. Along with these fine and interesting specimens was one which at once struck me as different from any I had seen ; this I did not hesitate to beg, and I was kindly allowed to have it for examination. On looking over some nodules subsequently received from the same place, I found two which resembled Mr. Brown's so closely that I proceeded to analyze them. The results of my investigation showed that the unfamiliar nodules consisted of a new mineral containing much sulphuric acid in addition to the elements of silicoborocalcite.

The nodule first examined (that from Mr. Brown's cabinet) weighed about 470 grains ; it had nearly the size and outline of a walnut, and small colourless crystals of considerable lustre covered the greater part of its exterior ; a small portion consisted of earthy gypsum ; the rest of the mass was colourless and translucent. The fracture was nearly flat, and the new surfaces were covered with glistening irregular facets. Under the microscope, scrapings were seen to be transparent oblique-angled plates. The hardness of the interior was about 3, that of the exterior about 2. In the closed tube, fragments gave water and became opaque ; before the blowpipe, decrepitated and fused readily to a clear bead, giving from the first a bright green flame ; on continued blowing, frothing took place, the head became opaque, and the flame was no longer coloured green. Hence the blowpipe reactions partake of those* which are so characteristic of Howlite, as pointed out in my original description (*Phil. Mag.* January 1868), and those of selenite. The following results were obtained from the air-dry mineral ; the old nomenclature and notation, still commonly used in mineralogy, are retained:—

	No. I.
Water	18·80
Lime	31·66
Sulphuric acid	36·10
Silicic acid	3·31
Boracic acid (by difference)	[10·13]
	100·00

* It is curious that no reference is made to these well-marked reactions in the description of this species in Dana's '*Mineralogy*,' fifth edition, p. 598 ; in cases of possible doubt they promptly distinguish Howlite from Ulexite.

These numbers lead to a formula including these elements,



as seen in the subjoined comparison :—

Calculated.				Found.
20HO	. . =180	18.30		18.80
11CaO	. . =308	31.30		31.66
9SO ³	. . =360	36.58		36.10
SiO ²	. . = 30.81	3.14		3.31
3BO ³	. . =105.12	10.68		[10.13]
	983.93	100.00		100.00

which brings out the fact that there is close accordance between the theoretical and the experimental percentages.

The second nodule analyzed was sent to me from Winkworth. It had not so crystalline an exterior as that just described : the upper part consisted of selenite, the lower of a layer of earthy gypsum about an eighth of an inch in thickness ; the intervening portion, forming the greater part of the whole nodule, which was about the size of the foregoing and weighed nearly 500 grains, had characters agreeing closely with those just described. The hardness was a little above 3 ; before the blowpipe the flame was coloured green, and a bead clear at the edges was obtained at first ; on blowing for some time, continuous bubbling, with the gradual shrinking of the bead to an opaque coating on the wire, took place. Scrapings had just the appearance of those of No. 1 under the microscope. In the analysis a little carbonate of calcium was found ; this was deducted, the amount (4.47 per cent.) being calculated from the quantity of carbonic acid obtained. The percentages on the air-dried mineral were these :—

		No. II.
Water	18.00
Lime	31.14
Sulphuric acid	31.51
Silicic acid	4.98
Boracic acid (by difference)		[14.37]
		100.00

which lead to a formula including these terms,

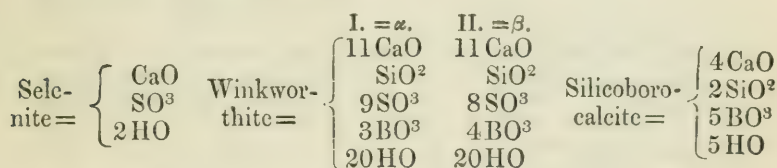


as appears on comparing the theoretical and experimental percentages, thus :—

			Calculated.		Found.
20HO	.	.	=180.00	18.40	18.00
11CaO	.	.	=308.00	31.47	31.14
8SO ³	.	.	=320.00	32.69	31.51
SiO ²	.	.	= 30.81	3.14	4.98
4BO ³	.	.	=140.16	14.30	[14.37]
			978.97	100.00	100.00

The agreement here, though not quite so close as in the preceding case, is sufficient to show that the formula chosen most probably represents the composition of the substance analyzed; and the resemblance of the results to those from No. 1 proves that we have to do with closely related minerals. In fact I view the second as theoretically derived from the first by the interchange of one atom of boracic acid and a little silica for one of sulphuric acid, and as being a variety of the species for which I propose the name Winkworthite.

This new species is intermediate between selenite and silicoborocalcite (Howlite), thus :—



and it may originate from the reaction of their elements during or after deposition. The nodular form, which is the characteristic of Howlite as contradistinguished from the crystalline habitude of selenite, seems to show that the former was the original deposit, in which case Winkworthite would be a product of its alteration by selenite. Nodules of selenite occasionally occur in the gypsum-beds here resembling exactly the amorphous forms of the borates, of which they are probably the pseudomorphs. I have examined several specimens of Howlite for sulphuric acid: in some cases mere traces were present, both in the hard crystalline and the soft opaque conditions of the mineral; while in other specimens, respectively similar, the quantity has been decided and sometimes considerable, even after every precaution had been taken to exclude selenite, which, as mentioned in my original description (Phil. Mag. Jan. 1868), is often present in bands traversing the borate.

In describing the distinct acicular prisms as a new form of natroborocalcite (Ulexite, Dana), I named the locality Newport (Phil. Mag. April 1870); but it should have been called Newport

Station. The fact is, the quarries affording this new and beautiful form of the mineral are in the township of Windsor, near, however, the boundary line between this and the township of Newport; and the name of the latter got to be associated with the quarries from its being that of the railway-station in their immediate vicinity, from which the gypsum is despatched to be shipped at Windsor. I have lately observed that this locality occasionally affords, by the side of the separate needles of Ulexite, the hard form of the same mineral, hardness = 3 (Phil. Mag. January 1868), and that the interior of the hard nodules is sometimes very like chalcedony in appearance. Last summer I found a good many specimens of Howlite in gypsum from Newport Station, mostly of the chalky variety; a few exhibited in an inferior degree the pearly scales of the Winkworth nodules; so that the borate is not so rare there as it was thought to be.

I add a summary of the localities of the borates in this county at present known to me, and the composition and matrix of each mineral.

Localities of Borates in Hants County, Nova Scotia.

<i>Borate and Matrix.</i>	<i>Localities.</i>
1. Natroborocalcite (Ulexite, Dana), NaO, 2CaO, 5BO ³ , 15HO, in gypsum, at	Clifton Quarry, Windsor; Brookville; Trecothick's Quarry, Three Mile Plains; Winkworth; Newport Station.
2. Cryptomorphite, NaO, 3CaO, 9BO ³ , 12HO, in glauber-salt in gypsum, at	
3. Silicoborocalcite (Howlite, Dana), 4CaO, 2SiO ² , 5BO ³ , 5HO, in gypsum, at	Brookville; Winkworth; Newport Station; Noel (Cheverie and Walton reported, but matrix unknown to me).
in anhydrite, at	
4. Winkworthite, α. 11CaO, SiO ² , 9SO ³ , 3BO ³ , 20HO, β. 11CaO, SiO ² , 8SO ³ , 4BO ³ , 20HO, in gypsum, at	Brookville. Winkworth.

XXXVI. *On the Light from the Sky, its Polarization and Colour.*
By the Hon. J. W. STRUTT, Fellow of Trinity College, Cambridge.

[Continued from p. 120.]

IN the February Number of the Philosophical Magazine I have propounded a theory of the scattering of light by particles which are small in *all* their dimensions compared with the

wave-length of light, and have applied the results to explain the phenomena presented by the sky. Another theory has been given by Clausius, who attributes the light of the sky to reflection from water-bubbles, and has developed his views at length in a series of papers in Poggendorff's *Annalen* and Crelle's *Journal**.

Starting from the ordinary laws of reflection and refraction, he has no difficulty in showing that, were the atmosphere charged with globes of water in sufficient quantity to send us the light which we actually receive, a star instead of appearing as a point would be dilated into a disk of considerable magnitude. But the requirements of the case are satisfied if we suppose the spheres hollow, like bubbles; for then, on account of the parallelism of the surfaces, but little effect is produced by refraction on a wave of light. At the same time, if the film be sufficiently thin, the light reflected from it will be the blue of the first order, and so the colour of the sky is apparently accounted for.

Apart from the difficulty of seeing how such bubbles could be formed, there is a formidable objection to this theory, mentioned by Brücke (Pogg. *Ann.* vol. lxxxviii. p. 363)—that the blue of the sky is a much better colour than the blue of the first order. That it is so appears clearly from the measurements quoted in the February Number, and from the theoretical composition of the blue of the first order†. Nor can we escape from this difficulty by supposing, with Brücke, that the greater part of the light from the sky has been reflected more than once.

Brücke also brings forward an experiment of great importance when he shows that mastic precipitated from an alcoholic solution scatters light of a blue tint. He remarks that it is impossible to suppose that the particles of mastic are in the form of bubbles.

In his last utterance on this subject‡, Clausius replies to the objections urged by Brücke and others against his theory, and shows that, if the illumination of the sky is due to thin plates at all, those thin plates *must* be in the form of bubbles. While ad-

* Pogg. *Ann.* vols. lxxii. lxxvi. lxxxviii. Crelle, vols. xxxiv. xxxvi.

† I find that I omitted to explain why it is that the light dispersed from small particles is of so much richer a hue than that reflected from very thin films. In the latter case the reflected wave may be regarded as the sum of the disturbances originating in the elementary parts of the film, and these elementary parts may be assimilated to the small particles of the former supposition. The integration is best effected by dividing the surface into the *zones of Huyghens*; and it is proved in works on physical optics that the total effect is just half of that due to the first zone. Now the zones of Huyghens vary as the wave-length; and thus it appears that in the integration the long waves gain an advantage which diminishes the original preponderance of their quicker-timed rivals.

‡ Pogg. *Ann.* vol. lxxxviii. p. 543.

mitting that if the particles are very small the ordinary laws of reflection and refraction no longer apply*, and that therefore this case is not excluded by his argument, he still holds to his original view as to the nature of the reflecting matter in the sky, considering that the polarization of the light indicates that it has undergone *regular* reflection. His concluding paragraph so well sums up the case that I cannot do better than quote it. "Das Resultat der vorstehenden Betrachtungen kann ich hienach kurz so zusammenfassen. Soweit man die gewöhnlichen Brechungs- und Reflexionsgesetze als gültig anerkennt, glaube ich auch meine früheren Schlüsse festhalten zu müssen, nämlich, dass in der Atmosphäre Dampfbläschen vorhanden seyen, und dass sie die Hauptsache der in ihr stattfindenden Lichtreflexion und ihrer Farben bilden. Nimmt man aber an, die in der Atmosphäre wirksamen Körperchen seyen so klein, dass jene Gesetze auf sie keine Anwendung mehr finden, dann sind auch diese Schlüsse ungültig. Auf diesen Fall ist aber auch die Theorie der Farben dünner Blättchen nicht mehr anwendbar, und er bedarf vielmehr einer neuen Entwicklung, bei welcher noch besonders berücksichtigt werden muss, in wiefern diese Annahme mit der Polarisation des vom Himmel kommenden Lichtes und mit der angenähert bekannten Grösse der in den Wolken vorhandenen Wassertheilchen vereinbar ist."

Clausius does not seem to have followed up the line of research here indicated. My investigation (written, it so happens, before seeing Clausius's papers) shows in the clearest manner the connexion between the smallness of the particles and the polarization of the light scattered from them. Indeed I must remark that in this respect there is an advantage over the theory of thin plates, according to which the direction of complete polarization would be about 76° from the sun. It would be a singular coincidence if the action of secondary causes were to augment this angle to 90° —its observed magnitude. It seems, therefore, not too much to say that, if the illumination of the sky were due to suspended water-bubbles, neither its colour nor its polarization would agree with what is actually observed.

In his celebrated paper on Fluorescence†, Professor Stokes makes the following significant remark:—"Now this result appears to me to have no remote bearing on the question of the direction of the vibrations in polarized light. So long as the

* In many departments of science a tendency may be observed to extend the field of familiar laws beyond their proper limits. Thus the properties of gross matter are often assumed to hold equally good for molecules. An example more analogous to that which suggests this remark is to be found in the common explanation of the mode of action of the speaking-trumpet.

† Phil. Trans. 1852, p. 526.

suspended particles are large compared with the waves of light, reflection takes place as it would from a portion of the surface of a large solid immersed in the fluid, and no conclusion can be drawn either way. But if the diameter of the particles be small compared with the length of a wave of light, it seems plain that the vibrations in a reflected ray cannot be perpendicular to the vibrations in the incident ray." This is the only passage that I have met with in which the theory of the reflection of light from very small particles is touched upon.

If it be assumed, as in the theories of Green and Cauchy of reflection at plane surfaces, that the effect of dense matter is merely to load the ether, it follows rigorously that the direction of vibration cannot be turned through a right angle when light is scattered from small particles. But all we know in the first instance is that the velocity of propagation of luminous waves is less in ordinary transparent matter than in vacuum; and this may be accounted for as well by a diminished rigidity as by an increased density. In the first case a scattered ray *might* be composed of vibrations perpendicular to those of the incident beam; so that the matter is not quite so clear as it would seem from the argument of Professor Stokes. I believe, however, that good reasons may be given for rejecting the view that the difference between media of varying refrangibility is one of rigidity. The point is an important one, and I propose to recur to it later.

The experiments of Professor Tyndall* with precipitated clouds exhibit more clearly than had been done by Brücke the relation between the size of the particles and the nature of the dispersed light. The observation that the polarization is complete perpendicular to the track of the incident light is in itself sufficient to disprove the theory of bubbles. As the particles increase in magnitude, the azure and polarization are gradually lost. During the transition a different and more complicated set of phenomena present themselves, which will furnish a test for the theory when it is extended so as to include the consideration of particles which are no longer very small in comparison with the waves of light.

All who have written on this subject seem to have taken for granted that the foreign matter in the atmosphere is water or ice. Even Tyndall, who expressly says that any particles, if small enough, will do, still believes in the presence of water-particles. But this view is encumbered with considerable difficulty; for even if, in virtue of its transparency to radiant heat, the air in the higher regions of our atmosphere is at a very low temperature, it would still be capable of absorbing the very small quantity of water which is sufficient to explain the blue of the

* Phil. Mag. vol. xxxvii. p. 385. Phil. Trans. 1870.

Phil. Mag. S. 4. Vol. 41. No. 273. April 1871.

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sky. At any rate it is difficult to imagine particles of water smaller than the wave-length endowed with any stability. These difficulties might perhaps be got over if there were any strong argument in favour of the water-particles; but of the existence of such I am not aware. Every one knows that a blue haze evidently akin to the azure of the sky obliterates the details and modifies the colour of a distant mountain; and this, when it occurs on a hot day, cannot possibly be attributed to aqueous particles. On the face of it, there is no reason for supposing that near the earth's surface the foreign matter is of one kind and at a great altitude another. If it were at all probable that the particles are all of one kind, it seems to me that a strong case might be made out for common salt. Be this as it may, the optical phenomena can give us no clue.

The apparatus by means of which the comparison was made between sky light and that of the sun diffused through white paper, was originally arranged for measurements of the absolute absorption of coloured fluids for the various rays of the spectrum, and had been applied rather extensively in experiments having that object. In the shutter of a darkened room were placed two slits in the same vertical line, each about three inches long, and a foot apart. At the other end of the room was an arrangement of prisms and lenses for producing a pure spectrum on a screen in the ordinary way. At first only one prism was used; but I soon introduced another; and the number might probably be further increased with advantage. It is even more important to have a great dispersion in these experiments than in the ordinary spectroscope. Two spectra would thus be thrown on the screen one over the other, but by means of a very obtuse-angled prism situated in front of the dispersion-prisms they are brought together so as exactly to overlap. The double spectrum thus formed passes through a horizontal slit in the screen placed so as to receive it. Close behind is an opaque card carrying a small vertical slit, which can be slid along so as to allow any desired part of the spectrum to pass through. At the beginning and end of a set of experiments the card is removed, and the principal fixed lines are observed through an eyepiece and referred to a scale situated just over the horizontal aperture.

When the experimenter looks through the eye-slit in the direction of the lens, he sees the two parts of the obtuse prism illuminated with light, in each case homogeneous and, if the adjustments are properly made, belonging to the same part of the spectrum. By varying the breadths of the original slits, the two parts of the field may be made equally bright; and when the match is attained, the breadths are inversely proportional to the richness of the lights behind them in the homogeneous ray under

consideration. But if the object be to make a complete comparison between two lights, it is often more convenient to leave the widths of the slits arbitrary, and then, by sliding the card, to seek that part of the spectrum which allows a match. It was in this way that the observations on the light of the sky were made. To give an idea of the degree of accuracy to which the comparisons may be made, I may mention that in my experiments on absorption, the means of six observations were usually correct to about one in 50 or 60. In the less-luminous parts of the spectrum the error might be somewhat greater.

The difficulty, however, of getting a satisfactory result with the blue of the sky does not lie in the inaccuracy of the measurements, but in the arbitrary character of the light with which it is compared. In order to test the theory in a strict manner, the second light ought to be similar in composition to that which lights up the sky. Now the sky is lit not only by the direct rays of the sun, but also by itself and by the bright surface of the earth. It is evident, therefore, that the requirements of the case are very imperfectly met by taking as the second light that of the sun as received by us, even if the translucent material through which we diffuse it effects no change in the quality. A nearer approximation to what we want would probably be found in the diffused light of a thoroughly cloudy day. But here we meet with an experimental difficulty; for the method described is only available to compare two lights both given at once. A suitable artificial light might no doubt be used as a middle term to be afterwards eliminated; but a candle or a lamp would hardly be available, on account of the yellowness of their light. On the other hand, the bluer radiation from burning magnesium would probably be inconvenient, and difficult to keep constant in quality from day to day. I am, however, in hopes that, by a method founded on a different principle, I may be able to compare the blue of one day's clear sky with the white light from the clouds on another.

Terling Place, Witham,
March 6, 1871.

XXXVII. *On Attraction caused by Vibrations of the Air.*
By Professor CHALLIS, M.A., F.R.S., F.R.A.S.*

AFTER concluding from reasoning founded on hydrodynamical principles, in my work '*On the Principles of Mathematics and Physics*,' that the vibrations of an elastic fluid, such as the air, are capable of causing a permanent motion of

* Communicated by the Author.

($d\phi$) for this differential, we have

$$\begin{aligned}\frac{(d\phi)}{ds} &= u \frac{dx}{ds} + v \frac{dy}{ds} + w \frac{dz}{ds} \\ &= u \cdot \frac{u}{V} + v \cdot \frac{v}{V} + w \cdot \frac{w}{V} = V.\end{aligned}$$

Hence

$$\frac{dV}{dt} = \frac{d^2\phi}{ds dt}, \text{ and } \int \frac{dV}{dt} ds = \frac{d\phi}{dt}.$$

By integrating the equation (α),

$$a'^2 \text{ Nap. log } \rho = - \int \frac{dV}{dt} ds - \frac{V^2}{2} + f(\alpha, \beta, \gamma, t),$$

α, β, γ being the coordinates of a given point on the line s . Now, if we suppose that the mass of fluid is of unlimited dimensions, and that at a certain position it is agitated by the vibrations of a small body, on tracing from the source of disturbance the course, at any given instant, of any line s coinciding throughout in direction with that of the motion, a position will be reached at which the motion is of insensible magnitude; and this will be the case even if the course of s should be modified by incidence of the vibrations on a small solid obstacle. At every such position $\frac{dV}{dt} = 0$, $V = 0$, and the density is that of the fluid at rest.

Calling this density ϖ , we shall have

$$a'^2 \text{ Nap. log } \varpi = f(\alpha, \beta, \gamma, t),$$

and

$$\therefore a'^2 \text{ Nap. log } \frac{\rho}{\varpi} = - \int \frac{dV}{dt} ds - \frac{V^2}{2}.$$

Hence we obtain the exact equation

$$\frac{\rho}{\varpi} = e^{-\frac{1}{2} \int \frac{dV}{dt} ds - \frac{V^2}{2a'^2}};$$

consequently, by expanding to terms of the second order, and putting $\varpi(1 + \sigma)$ for ρ ,

$$a'^2 \sigma = - \int \frac{dV}{dt} ds - \frac{V^2}{2} + \frac{1}{2a'^2} \left(\int \frac{dV}{dt} ds \right)^2.$$

Now the only part of the pressure $a'^2 \sigma$ which can give rise to a motion of translation is expressed by the non-periodic terms of the right-hand side of this equation. Hence, omitting the term

$\int \frac{dV}{dt} ds$, which, as we have seen, is wholly periodic whether or not it contains small quantities of the second order, and putting

in the last term for $\int \frac{dV}{dt} ds$ its first approximation $\frac{d\phi}{dt}$, all non-periodic terms of the *second* order will be contained in the expression

$$-\frac{V^2}{2} + \frac{1}{2a'^2} \frac{d\phi^2}{dt^2}.$$

The finding of such terms requires, therefore, a previous determination of the values of V and $\frac{d\phi}{dt}$. I proceed to do this in a simple instance.

It will be supposed that the vibrations propagated in the fluid are functions of the distance from a fixed point, being continuously impressed at a given small distance from the point equally in all directions. Although these circumstances differ greatly from those of the vibrations in Mr. Guthrie's experiments, which were excited by a tuning-fork, the discussion of this case, which has been selected on account of the simplicity of the requisite analytical calculation, may serve to indicate how the "approach" was caused in the instances of those experiments, and generally how a motion of translation can result from the action of aërial vibrations.

The distance from the fixed point being called r , the solution of the known equation applicable to this case, viz.

$$\frac{d^2 \cdot r\phi}{dt^2} = a'^2 \frac{d^2 \cdot r\phi}{dr^2},$$

gives, on the supposition that the vibrations are propagated from the centre,

$$V = \frac{f'(r - a't + c)}{r} - \frac{f(r - a't + c)}{r^2},$$

$$a'\sigma = \frac{f'(r - a't + c)}{r} = -\frac{d\phi}{a'dt}.$$

Giving now to the function f the form $-m \cos \frac{2\pi}{\lambda} (r - a't + c)$, we have

$$f'(r - a't + c) = \frac{2\pi m}{\lambda} \sin \frac{2\pi}{\lambda} (r - a't + c),$$

$$V = \frac{2\pi m}{\lambda r} \sin \frac{2\pi}{\lambda} (r - a't + c) + \frac{m}{r^2} \cos \frac{2\pi}{\lambda} (r - a't + c),$$

and, by supposing that $\frac{\lambda}{2\pi r} = \tan \theta$,

$$V = \frac{2\pi m}{\lambda r} \left(1 + \frac{\lambda^2}{4\pi^2 r^2}\right)^{\frac{1}{2}} \sin \left(\frac{2\pi}{\lambda} (r - a't + c) + \theta\right).$$

Also

$$\frac{d\phi}{a'dt} = -\frac{f'(r-a't+c)}{r} = -\frac{2\pi m}{\lambda r} \sin \frac{2\pi}{\lambda} (r-a't+c).$$

Hence the non-periodic part of $-\frac{V^2}{2} + \frac{d\phi^2}{2a'^2 dt^2}$ is

$$-\frac{1}{4} \left\{ \frac{4\pi^2 m^2}{\lambda^2 r^2} \left(1 + \frac{\lambda^2}{4\pi^2 r^2} \right) - \frac{4\pi^2 m^2}{\lambda^2 r^2} \right\},$$

which is equal to $-\frac{m^2}{4r^4}$. Since this is a negative quantity, the pressure of the fluid is least where the velocity is greatest, so far as it depends on the non-periodic terms. Consequently, supposing a disk to be submitted to the action of the vibrations, since the motion will be greater on the face which directly receives them than on the opposite face, the disk will be apparently attracted toward the source of the vibrations, as in the known experiment of Clement. It is plain that bodies of other forms will be attracted, if only the condition be fulfilled of the velocity being greater on the side turned toward the source of the vibrations than on the opposite side.

Let it now be supposed that the vibrations are originally produced at the distance b from the centre, and that at this distance the velocity at any time t is expressed by $\mu \sin \frac{2\pi a't}{\lambda}$. Hence, putting b for r in the value of V , we must have

$$\mu \sin \frac{2\pi a't}{\lambda} = \frac{2\pi m}{\lambda b} \left(1 + \frac{\lambda^2}{4\pi^2 b^2} \right)^{\frac{1}{2}} \sin \left(\frac{2\pi}{\lambda} (b-a't+c) + \theta \right).$$

To satisfy this equation independently of the time t , it is sufficient to determine the arbitrary quantities m and c by the equations

$$\mu^2 = \frac{4\pi^2 m^2}{\lambda^2 b^2} \left(1 + \frac{\lambda^2}{4\pi^2 b^2} \right), \quad \frac{2\pi}{\lambda} (b+c) + \theta = \pi.$$

The second equation gives

$$\frac{2\pi c}{\lambda} = \pi - \frac{2\pi b}{\lambda} - \tan^{-1} \frac{\lambda}{2\pi b},$$

whereby c is expressed in terms of known quantities; and from the other equation

$$m^2 = \frac{\mu^2 b^4}{1 + \frac{\lambda^2}{4\pi^2 b^2}}.$$

Hence the sum of the non-periodic terms, which sum may be

considered to express the attractive action, is

$$-\frac{\mu^2 b^4}{4r^4 \left(1 + \frac{4\pi^2 b^2}{\lambda^2}\right)}.$$

From this formula it follows that the attraction is greater as the impressed velocity μ is greater, and that it decreases rapidly with an increase of the distance r . These inferences agree with the results of the experiments. It appears also from the mathematical theory that *cæteris paribus* the attraction is greater as λ is greater. In the description of the apparatus used in making the experiments the author states that the fork made 128 complete vibrations in a second; whence I infer that the value of λ was $8\frac{1}{2}$ feet. The distance b at which, as measured from a centre, the disturbance might be considered to be made, was very much less than this; so that the amount of attraction was little influenced by the value of λ .

In those experiments in which a fixed card or a fixed vibrating fork was placed near a moveable vibrating fork, the approach of the latter was still caused by the diminution of density in the intervening space due to the non-periodic part of the velocity. The explanation of the motion in these instances is analogous to the theoretical account I have given of *magnetic* attraction in the *Philosophical Magazine* for January 1861 (art. 7, p. 68), and in the 'Principles of Mathematics and Physics' (art. (4), p. 607).

In concluding this communication I wish to call attention to the circumstance that I have employed above the integral of the equation

$$\frac{d^2.r\phi}{dt^2} = a'^2 \frac{d^2.r\phi}{dr^2}$$

in a manner inconsistent with views I have repeatedly advocated in this Magazine, and more recently in pp. 251–254 of the above-cited work. I have argued, on grounds that admit of no dispute, that if the equation

$$a'\sigma = \frac{f'(r - a't + c)}{r}$$

be true, there cannot be a *solitary* wave of condensation, for in that case the condensation must vary inversely as the *square* of r ; or, if the solitary wave be possible, that equation cannot be true. None of my mathematical contemporaries, as far as I am aware, have recognized the necessity of deciding between these two views, although, till this be done, Hydrodynamics is hardly en-

titled to be called a science. For a long time I admitted the possibility of the propagation of a solitary wave, and consistently therewith maintained that the condensation varied inversely as the square of the distance. But I have recently become aware that that admission is contradicted by results of my hydrodynamical researches which, I have reason to say, are well established. The contradiction I refer to will be understood from the following explanation.

A general law of free vibratory motion parallel and transverse to an axis, and of uniform propagation of such motion in the direction of the axis, having been arrived at antecedently to the consideration of any cause of disturbance of the fluid, it seems necessary to conclude that motions produced by given arbitrary disturbances are actually composed of such spontaneous motions due exclusively to properties of the fluid. (I need not here refer more particularly to these views, further than to say that the arguments by which they are maintained were fully given in the pages of this Journal.) Now a solitary wave of condensation or rarefaction cannot be composed in this manner, because the result of such composition must be *both condensation and rarefaction*. It seems, therefore, that on these principles, when a disturbance tends to produce condensation only, the condensation is resolved into a series of alternate condensations and rarefactions, arranged in a manner depending on the disturbance on each side of a maximum condensation, and diminishing by gradations in both directions from this, in such manner that the excess of condensation is equal to that impressed by the disturbance. A solitary wave of rarefaction would be similarly resolved. Also like resolutions would take place at an abrupt beginning, and at an abrupt ending, of a regular series of plane-waves. Similar considerations may be applied to account for resolutions into series of *transverse* vibrations caused by abrupt *lateral* disturbances.

According to these views, the condensation in waves propagated from a centre, however they may have originated, will vary inversely as the distance. For, as I have elsewhere proved, and in fact may easily be shown, by reason of the part of the velocity expressed by the term $-\frac{f(r - at + c)}{r^2}$, the rate of diminution of the condensation or rarefaction with distance from the centre will be continually changed from the law of the inverse square of the distance to that of the simple inverse of the distance, *provided there be alternate condensations and rarefactions*. For in that case the above-mentioned velocity gives rise to a continual flow *from the rarefied into the condensed parts*, and just in the proportion required for altering the law of diminution with

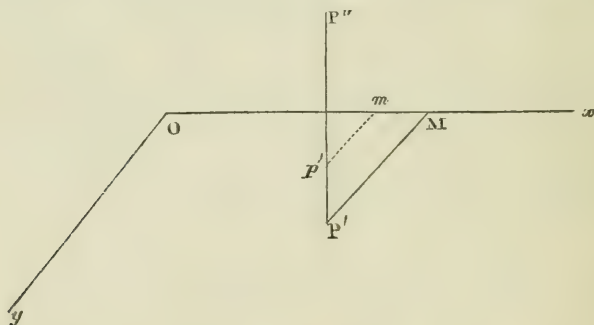
the distance from the inverse square to the simple inverse. I forbear going further into this subject at present, and shall only remark further that these views bear in an important manner on the explanation by theory of certain phenomena of light, and on other parts of theoretical physics.

Cambridge, March 14, 1871.

XXXVIII. On the Plane Representation of a Solid Figure.

By Professor CAYLEY, F.R.S.*

WE represent *in plano* the position of a point P whose co-ordinates in space are (x, y, z) , by drawing these co-ordinates, on the same scale or on different scales, and in given directions from a fixed origin in the plane; $OM = x$, $MP' = y$, $P'P'' = z$. But observe that the point P'' alone does not completely represent the point P ; in fact P'' represents a whole series of points lying in a line; any one such point is the point



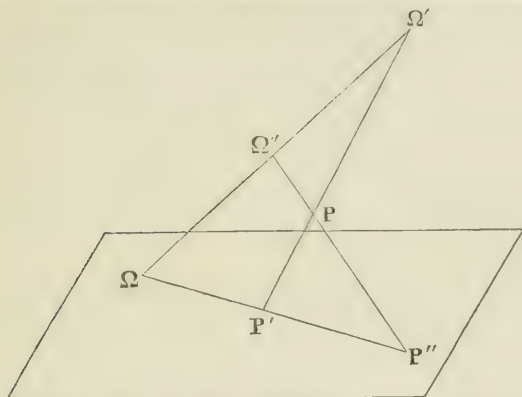
whose coordinates are $Om, mp', p'P''$. For the complete representation of P we require the *two points* P', P'' : these might be distinguished as the projection P'' , and the foot-point P' . The two points P', P'' are obviously such that the line joining them is in a given direction.

The preceding is, of course, the ordinary method of orthogonal projection, or geometrical delineation of a solid figure: it may be used under various forms; for example, the coordinates x, y, z may be taken on the same scale and in directions inclined to each other at angles of 120° (isometrical projection); or the coordinates x, y may be drawn on the same scale and at their actual inclination, 90° , to each other; and the coordinate z on the same or an altered scale in any given direction; the points P' then give a true ground-plan of the solid figure, and the lengths

* Communicated by the Author.

of the lines $P'P''$ give the altitudes of the several points P : this is also a method in ordinary use.

But it is to be observed that the points P', P'' are both of them



projections, and that the general theory is as follows: we represent the position of the point P by means of its projections P', P'' , from two fixed points Ω', Ω'' respectively; the line joining these points passes, it is clear, through a fixed point Ω which is the intersection of the plane of projection by the line which joins the two points Ω', Ω'' .

Hence we say that a point P in space is represented *in plano* by any two points P', P'' which are such that the line joining them passes through a fixed point Ω . And we have thus a *system of constructive geometry* which is the more simple on account of the generality of its basis, and which is at once applicable to any of the special projections above referred to. I establish the fundamental notions of such a geometry, and by way of illustration apply it to the solution of the well-known problem of finding the lines which meet four given lines in space.

A point P (as already mentioned) is given by its projections P', P'' , which are points such that the line joining them passes through the fixed point Ω .

A line L is given by its projections L', L'' , which are any two lines in the plane. We speak of the point (P', P'') , meaning the point P whose projections are P', P'' ; and similarly of the line (L', L'') , meaning the line whose projections are L', L'' .

If P', P'' coincide, then the point P is in the plane of projection; and so if L', L'' coincide, then the line L is in the plane of projection.

If through Ω we draw a line meeting L', L'' in the points P', P'' respectively, these are the projections of a point P on the line L . In particular the intersection of L', L'' (considered as

two coincident points) represents the intersection of the line L with the plane of projection.

The line through the points (P', P'') and (Q', Q'') has for its projections the lines $P'Q'$ and $P''Q''$.

Two lines (L', L'') and (M', M'') intersect each other if only the intersections $L' M'$ and $L'' M''$ are the projections of a point P —that is, if the line through the points $L' M'$ and $L'' M''$ passes through Ω . And then clearly P is the intersection of the two lines.

A plane Π is conveniently given by means of its trace Θ on the plane of projection, and of the projections (P', P'') of a point on the plane; or, say, by means of the trace Θ , and of a point P on the plane.

Suppose, however, that a plane is given by means of a line L and a point P on the plane. The trace Θ passes through the point of intersection of the line L with the plane of projection—that is, through the point of intersection of the projections L', L'' . To find another point on the trace, we have only to imagine on the line L a point Q , and, joining this with P , to suppose the line PQ produced to meet the plane of projection. The construction is obvious; but by way of illustration I give it in full. Through Ω draw a line meeting L', L'' in Q', Q'' respectively (then these are the projections of a point Q on the line L); the lines $P'Q'$ and $P''Q''$ are the projections of the line PQ , and the intersection of $P'Q'$ and $P''Q''$ is therefore the required point on the trace Θ .

The line of intersection of two planes passes through the point of intersection of their traces Θ_1, Θ_2 ; whence, if the planes have in common a point P , the line of intersection is the line joining P with the intersection of the traces Θ_1, Θ_2 .

In what precedes we have the solution of the following problem:—"Given a point P , and two lines L_1, L_2 , to find a line through P meeting the two lines L_1, L_2 ." The required line is in fact the line of intersection of the planes (P, L_1) and (P, L_2) ; we have seen how to construct the traces Θ_1 and Θ_2 of these planes respectively; and the required line is the line joining P with the intersection of Θ_1 and Θ_2 .

I proceed now to the problem to find the two lines, each of them meeting four given lines, L_1, L_2, L_3, L_4 (these being, of course, given by means of their projections (L'_1, L''_1) &c.). The question is in effect to find on the line L_1 a point P such that, drawing from it a line to meet L_2, L_3 , and also a line to meet L_2, L_4 , these shall be one and the same line.

Now, considering in the first instance P as an arbitrary point on the line L_1 , the line from P to meet L_2, L_3 is any line whatever meeting the lines L_1, L_2, L_3 : say it is a generating line of

the hyperboloid whose directrices are L_1, L_2, L_3 , or of the hyperboloid $L_1 L_2 L_3$. Hence projecting from any point Ω' whatever, the generating lines and directrices are projected into tangents of one and the same conic. We know the projections L'_1, L'_2, L'_3 of the directrices; to find two other tangents of the conic, we take two arbitrary positions of P on the line L_1 , and construct as above the projections M', N' of the lines from these to meet the lines L_2, L_3 . The conic is then given as the conic touching the five lines, L'_1, L'_2, L'_3, M', N' : say this is the conic Σ' . Similarly, instead of Ω' , considering the point Ω'' , we have the lines L''_1, L''_2, L''_3 and the lines M'', N'' , which are the other projections of the lines through the two positions of P ; and touching these five lines we have a conic Σ'' . Each tangent T' of Σ' , combined with the *corresponding* tangent T'' of Σ'' , represents a line T meeting L_1, L_2, L_3 ; to establish the correspondence, observe that, inasmuch as the line T meets L_1 , the intersections of T', L'_1 and of T'', L''_1 must lie in a line with Ω ; if T' be given, the point T'' is thus uniquely determined, and therefore also T'' (since L''_1 is a tangent of Σ''); and similarly if T'' be given, T' is uniquely determined; the correspondence T', T'' is thus, as it should be, a (1, 1) correspondence.

Considering in like manner the lines which meet L_1, L_2, L_4 , we have touching $L'_1, L'_2, L'_4, \bar{M}', \bar{N}'$ a conic $\bar{\Sigma}'$; and touching $L''_1, L''_2, L''_4, \bar{M}'', \bar{N}''$ a conic $\bar{\Sigma}''$; each tangent \bar{T}' of $\bar{\Sigma}'$, combined with the corresponding tangent \bar{T}'' of $\bar{\Sigma}''$, represents a line meeting L_1, L_2, L_4 , the correspondence being a (1, 1) correspondence such as in the former case.

The conics $\Sigma', \bar{\Sigma}'$ both touch L'_1, L'_4 ; hence they have in common two tangents. Say one of these is $T' = \bar{T}'$, the corresponding tangents T'' and \bar{T}'' will coincide with each other and be a common tangent of $\Sigma'', \bar{\Sigma}''$ (these conics both touch L''_1, L''_4 , and have thus in common two tangents). We have thus $T' = \bar{T}'$, and $T'' = \bar{T}''$ as the projections of a line meeting L_1, L_2, L_3, L_4 ; and taking the other common tangents of $\Sigma', \bar{\Sigma}'$ and of $\Sigma'', \bar{\Sigma}''$, we have the projections of the other line meeting L_1, L_2, L_3, L_4 .

The whole process is:—Construct M', M'' and N', N'' , each of them the projections of a line through a point P of L_1 , which meets L_2, L_3 ; and \bar{M}', \bar{M}'' and \bar{N}', \bar{N}'' , each of them the projections of a line through a point P of L_1 , which meets L_2, L_4 ; we have then the conics

Σ', Σ'' touching L'_1, L'_2, L'_3, M', N' , and $L''_1, L''_2, L''_3, M'', N''$
respectively,

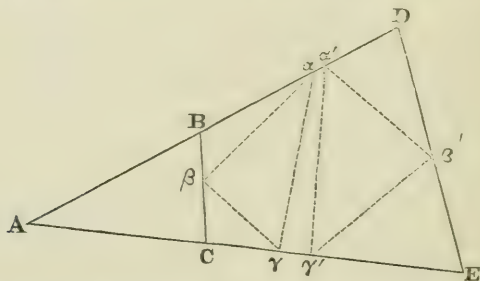
$\bar{\Sigma}', \bar{\Sigma}''$ „ $L'_1, L'_2, L'_4, \bar{M}', \bar{N}'$, „ $L''_1, L''_2, L''_4, \bar{M}'', \bar{N}''$
respectively;

and then the projections of each of the required lines are $T' = \bar{T}'$, a common tangent of Σ' , $\bar{\Sigma}'$, and $T'' = \bar{T}''$, the corresponding common tangent of Σ'' , $\bar{\Sigma}''$.

It is material to remark how the construction is simplified when there is given one of the lines, say, M , which meets L_1, L_2, L_3, L_4 . Here M is a common directrix of the two hyperboloids; we may for the hyperbolas Σ' and Σ'' consider, instead of L_1, L_2, L_3 and two new generating lines, the lines L_1, L_2, L_3, M , and a single new generating line N ; and similarly for the hyperbolas $\bar{\Sigma}', \bar{\Sigma}''$ the lines L_1, L_2, L_3, M and a single new generating line \bar{N} . $\Sigma', \bar{\Sigma}'$ have thus in common the three tangents L'_1, L'_2, M' , and therefore only a single other common tangent, $T' = \bar{T}'$; and similarly $\Sigma'', \bar{\Sigma}''$ have in common the three tangents L''_1, L''_2, M'' , and therefore only a single other common tangent, $T'' = \bar{T}''$; and we have thus the other line cutting the four given lines.

I take the opportunity of mentioning the following theorem: "If in a given triangle we inscribe a variable triangle of given form, the envelope of each side of the variable triangle is a conic touching the two sides (of the given triangle) which contain the extremities of the variable side in question."

We have thence a solution of the problem (*Principia*, Book I. Sect. V. Lemma XXVII.), in a given quadrilateral to inscribe a quadrangle of given form. The question in effect is in the triangle ABC to inscribe a triangle $\alpha\beta\gamma$ of given form; and in the triangle ADE a triangle $\alpha'\beta'\gamma'$ of given form such that the



sides $\alpha\gamma, \alpha'\gamma'$ may be coincident. The envelope of $\alpha\gamma$ is a conic touching AD, AE , and the envelope of $\alpha'\gamma'$ a conic also touching AD, AE : there are thus two other common tangents, either of which may be taken for the position of the side $\alpha\gamma = \alpha'\gamma'$; and the problem thus admits of two solutions.

XXXIX. *On the Cause of the Interrupted Spectra of Gases.*

By G. JOHNSTONE STONEY, M.A., F.R.S., &c.*

IN the Philosophical Magazine for August 1868, there is a paper "On the Internal Motions of Gases"†, by the author of the following communication, in which a comparison is instituted between these motions and the phenomena of light, from which the conclusion is drawn that the lines in the spectra of gases are to be referred to periodic motions within the individual molecules, and not to the irregular journeys of the molecules amongst one another.

Mr. Stoney thinks it possible now to advance another step in this inquiry, and has given to the Royal Irish Academy an account, of which the following is an abstract, of the grounds upon which he founds this hope.

A *pendulous* vibration, according to the meaning which has been given to that phrase by Helmholtz, is such a vibration as is executed by the simple cycloidal pendulum. It is, accordingly, one in which the relation between the displacement of each particle and the time is represented by the simple curve of sines, of which the equation is

$$y = C_0 + C_1 \sin(x + \alpha),$$

where $y - C_0$ is the displacement of the particle from its central position; C_1 is the amplitude of the vibration; x stands for $2\pi \frac{t}{\tau}$, where t is the time from a fixed epoch, and τ the period of a complete double vibration; and α is a constant depending on the phase of the vibration at the instant which is taken as the epoch from which t is measured.

Now we may not assume that the waves impressed on the æther by one of the periodic motions within a molecule of a gas are of this simple character. We must expect them to be usually much more involved. And whatever may happen to be the intricacy of their form near to their origin, they will retain substantially the same complex character so long as they advance through the open undispersing æther, in which waves of all lengths travel at the same rate. But it would seem that a very different state of things must arise when the undulation enters a dispersing medium, such as glass.

Let us suppose that the undulation‡ before it enters the glass

* From the Proceedings of the Royal Irish Academy, read January 9, 1871. Communicated by the Author.

† In reading that paper, the reader is requested to correct 16^2 into $\sqrt{16}$ at the end of paragraph 2.

‡ By the term *undulation* is to be understood a series of waves.

consists of plane waves. Then, whatever the form of these waves, the relation between the displacement of an element of the æther and the time may be represented by some curve repeated over and over again. This curve may be either one continuous curve, or parts of several different curves joined on to one another. In the latter case (which includes the other) one of the sections of the curve may be represented by the equations

$$\left. \begin{aligned} y &= \phi_0(x) \text{ from } x=0 \text{ to } x=x_1, \\ y &= \phi_1(x) \text{ from } x=x_1 \text{ to } x=x_2, \\ \text{and so on to} \\ y &= \phi_t(x) \text{ from } x=x_t \text{ to } x=2\pi, \end{aligned} \right\} \dots \dots (1)$$

y being the displacement, and x being an abbreviation for $2\pi \frac{t}{\tau}$, where τ is the complete periodic time of one wave.

The undulation *in vacuo* will then be represented, according to Fourier's well-known theorem, by the following series :

$$\left. \begin{aligned} y &= A_0 + A_1 \cos x + A_2 \cos 2x + \dots \\ &+ B_1 \sin x + B_2 \sin 2x + \dots, \end{aligned} \right\} \dots \dots (2)$$

where the coefficients are obtained from equations (1) by the definite integrals

$$\left. \begin{aligned} \int_0^{2\pi} y \cos nx, dx &= \pi A_n, \\ \int_0^{2\pi} y \sin nx, dx &= \pi B_n. \end{aligned} \right\} \dots \dots (3)$$

Equation (2), the equation of the undulation before it enters the glass, may be put into the more convenient form

$$y - A_0 = C_1 \sin (x + \alpha_1) + C_2 \sin (2x + \alpha_2) + \dots, \dots (4)$$

where $y - A_0$ is the displacement from the position of rest, and the new constants are related to those of equation (2) as follows :

$$C_n = \sqrt{A_n^2 + B_n^2}, \quad \alpha_n = \tan^{-1} \frac{A_n}{B_n} \dots \dots (5)$$

The first term of expansion (4) represents a pendulous vibration of the full period τ ; the remaining terms represent harmonics of this vibration; *i. e.* their periodic times are $\frac{1}{2}\tau$, $\frac{1}{3}\tau$, &c. All of these also are pendulous; so that equation (4) is equivalent to the statement that whatever be the form of the plane undulation before entering the glass, it may be regarded as formed by the superposition of a number of simple pendulous vibrations, one of which has the full periodic time τ , while the others are harmonics of this vibration.

Moreover these vibrations will coexist in a state of mechanical independence of one another, if the disturbance be not too violent for the legitimate employment of the principle of the superposition of small motions. So long as the light traverses undispersing space these constituent vibrations will strictly accompany one another, since in open space waves of all periods travel at the same velocity. The general resulting undulation will therefore here retain whatever complicated form it may have had at first. But when the undulation enters such a medium as glass, in which waves of different periods travel at different rates, the constituent vibrations are no longer able to keep together, each being forced to advance through the glass at a speed depending on its periodic time. Thus there arises a physical resolution within the glass of series (4) into its constituent terms*. And if the glass be in the form of a prism, the pendulous undulations corresponding to the successive terms of series (4) will emerge in different directions, so that each will give rise to a separate line in the spectrum of the gas.

We thus find that one periodic motion in the molecules of the incandescent gas may be the source of a whole series of lines in the spectrum of the gas. The n th of these lines is represented by the term

$$C_n \sin (nx + \alpha_n),$$

in which C_n is the amplitude of the vibration ; and consequently C_n^2 represents the brightness of the line. If some of the coefficients of series (4) vanish, the corresponding lines are absent from the spectrum. This is analogous to the familiar case of the suppression of some of the harmonics in music, and appears to be what usually occurs in those spectra which are called by Plücker spectra of the Second Order.

* Other expansions similar to Fourier's series can be conceived, in which the terms, instead of representing pendulous vibrations, would represent vibrations of any other prescribed form ; and hence a doubt may arise whether the physical resolution effected by the prism is into the terms of the simpler series. That it is so may, perhaps, not be susceptible of demonstration ; but the following considerations seem to show it to be probable in so high a degree that it is the hypothesis which we ought provisionally to accept. For, first, the form of the emerging vibrations is independent of the material of the prism, since the lines correspond to the same wave-lengths as seen in all prisms ; and, secondly, it is independent of the amplitude of the vibration within very wide limits, since the positions of the lines remain fixed through great ranges of temperature, and, in many cases, when the temperature falls so low that the lines fade out through excessive faintness. The first consideration shows the series to be the same under varying circumstances ; and the second consideration suggests, as in the theory of the superposition of small motions, that this series is a series of *pendulous* vibrations.

In spectra of this kind the lines which fall within the limits of the visible spectrum appear at first sight to be scattered at irregular intervals. This may arise, and probably does in most cases arise in part, from the circumstance that there may be several distinct motions in each molecule of the gas, each of which produces its own series of harmonics in the spectrum, which by their being presented together to the eye give the appearance of a confused maze of lines. But it appears also to arise in part from the absence of most of the harmonics, so that it is not easy to trace the relationship between the few that remain. To do so without the assistance of spectra of the First Order, requires that we should have at our disposal determinations of the wave-lengths of the lines made with extraordinary accuracy; and perhaps in a few cases, as, for example, in the case of hydrogen, the marvellous determinations which have been made by Ångström may have the requisite precision.

The ordinary spectrum of hydrogen consists of four lines, corresponding to C in the solar spectrum, F, a line near G, and *h*. To these it is possible that we ought to add a conspicuous line in the solar prominences which lies near D, but which has not yet been found in the artificial spectrum of hydrogen. Of these lines, three, viz. C, F, and *h*, are to be referred to the same motion in the molecules of the gas.

○ In fact the wave-lengths of these lines, as determined by Ångström*, are:

$$h = 4101\cdot2 \text{ tenth-metres.}$$

$$F = 4860\cdot74 \quad ,,$$

$$C = 6562\cdot10 \quad ,,$$

These are their wave-lengths in air of standard pressure and 14° temperature, determined with extraordinary precision. We must correct these for the dispersion of the air, so as to arrive at the wave-lengths *in vacuo* which are proportionate to the periodic times. Now, by interpolating between Ketteler's observations† on the dispersion of air, we find

$$\mu_h = 1\cdot000 \ 29952,$$

$$\mu_F = 1\cdot000 \ 29685,$$

$$\mu_C = 1\cdot000 \ 29383$$

for the refractive indices of air of standard pressure and temperature for the rays *h*, F, and C. From these we deduce that if the air be at 14° of temperature, the refractive indices will become

* Ångström's *Recherches sur le Spectre Solaire*, p. 31. A tenth-metre means a metre divided by 10¹⁰; similarly a fourteenth-second is a second of time divided by 10¹⁴.

† Phil. Mag. 1866, vol. xxxii. p. 345.

$$\mu_h = 1.000\ 2845,$$

$$\mu_F = 1.000\ 2820,$$

$$\mu_C = 1.000\ 2791.$$

Multiplying the foregoing wave-lengths by these values, we find for the wave-lengths *in vacuo*,

$$h = 4102.37 \text{ tenth-metres,}$$

$$F = 4862.11 \quad ,,$$

$$C = 6563.93 \quad ,,$$

which are the 32nd, 27th, and 20th harmonics of a fundamental vibration whose wave-length *in vacuo* is

$$0.13127714 \text{ of a millimetre,}$$

as appears from the following Table :—

Observed wave-lengths reduced to wave-lengths <i>in vacuo</i> .	Calculated values.	Differences.
Xth-metres. $h = 4102.37$	Xth-metres. $\frac{1}{32} \times 131277.14 = 4102.41$	Xth-metres. +0.04
$F = 4862.11$	$\frac{1}{27} \times 131277.14 = 4862.12$	+0.01
$C = 6563.93$	$\frac{1}{20} \times 131277.14 = 6563.86$	—0.07

Thus the outstanding differences are all fractions of an eleventh-metre, an eleventh-metre being the limit within which Angström thinks that his measures may be depended on.

The wave-length 0.13127714 of a millimetre corresponds to the periodic time 4.4 fourteenth-seconds, if we assume the velocity of light to be 298,000,000 metres per second.

Hence we may conclude, with a good deal of confidence, that 4.4 XIVth-seconds is very nearly the periodic time of one of the motions within the molecules of hydrogen.

The other harmonics of this fundamental motion in the molecules of hydrogen, viz. the 19th, 21st, 22nd, &c. harmonics, are not found in this spectrum of hydrogen. But two other spectra of hydrogen are known to exist in which there are a great number of lines; and possibly the missing harmonics will be found amongst them when their positions shall have been sufficiently accurately mapped down. A far more moderate degree of accuracy will suffice in this case than was required by the foregoing investigation.

But it is from the examination of spectra of the First Order that the most copious results may be expected. These spectra consist of lines ruled close to one another, and presenting in the ag-

gregate the appearance of patterns which often resemble the flutings on a pillar. When these spectra are more carefully examined, it is probable that the whole series of lines occasioning one of the fluted patterns will be found to be the successive harmonics of a single motion in the molecules of the gas. It may readily be shown that such patterns as are met with in nature may in this way arise. For this purpose it is only necessary to make some suitable hypothesis as to the original undulation impressed by the gas upon the æther. Thus, if the law of this undulation were the same as that of the motion of a point near the end of a violin-string, and of a periodic time sufficiently long (as, for example, two million-millionths of a second), this undulation, when analyzed by the prism, would give a spectrum covered with lines ruled at intervals about the same as that between the two D lines, and of intensities varying so as to become gradually brighter and then gradually fainter several times in succession in passing from line to line along the spectrum. These alternations would give a fluted appearance to the spectrum; and from appropriate hypotheses as to the original vibration, all the patterns met with in nature would result. Possibly it may prove to be practicable to trace back from the appearances presented within the limits of the visible spectrum to the character of the original motion to which they are all to be referred. But, however this may be, it will be easy in a spectrum of this kind, in which we have a long series of consecutive harmonics, to determine at least the *period* of this motion; and it is in the examination of these spectra that the most easily obtained results may be expected. But the necessary observations are at present almost altogether wanting. The only case in which the author had been able to arrive at any result was that of the nitrogen spectrum of the First Order, observed by Plücker. It would appear from his observations* that the more refrangible of the two fluted patterns observed by him is due to a motion in the gas having a wave-length of about 0.89376 of a millimetre, which corresponds to a periodic time of three XIIth-seconds, one of the flutings consisting of the thirty-five harmonics from about the 1960th to the 1995th.

This result, however, does not command the confidence which the preceding determination of one of the periodic times in hydrogen does; but it will suffice to show the character of the much easier investigation which has to be made in the case of gases which produce spectra of the First Order.

* Philosophical Transactions for 1865, p. 7, § 16.

XI. *On the Relations of Magnetism and Static Electricity.* By
CHARLES W. VINCENT, *Temporary Assistant Librarian, Royal
Institution*.*.

FORCE, as we recognize it, is the conversion of power from one of its phases or manifestations into another; and in the act and at the time of this conversion the original force is so far changed in direction, exhausted, and finally extinguished, the sum of the new force or forces being the measure of the quantity and intensity of the original force. A continued exertion of the same force implies its renewal from some other source.

That a magnet, in the continued exertion of its force without apparent diminution (as, for instance, when suspended by its keeper it maintains itself through time against gravity, without such an apparent expenditure of power as would account for the work done), nevertheless follows the same law is, I think, the generally accepted view of physicists; but the source whence it obtains its renewal of expended power is as yet a matter for experimental investigation.

To the writer's mind the modes of magnetization suggest, to a certain extent, the direction in which such an investigation might be profitably pursued. The voltaic current, magneto-electric current, currents of static electricity, or simple vibration of a magnetic in a diamagnetic medium in a magnetic field, alike give rise to the magnetic force.

Now in each of these cases magnetism is the resultant, not of the force acting primarily, but of that which it evokes; there is no magnetism in the magnetic meridian, but diamagnetic polarity. The static, magneto-electric, or voltaic currents do not transmit any of the force by which they exist *per se*, but expend themselves in exciting a state of diamagnetic tension in the surrounding media, which in its turn creates the magnetic current, the amount and intensity of which at the time must be the measure of the forces which excited it. Each of the forces quoted, so far as its active power is concerned, is finite and dependent on the expenditure of some other force; but if steel be made the recipient of its inductive force, it is mechanically changed in its structure (as Joule's experiment proves) by the manner in which it makes its molecules take up and arrange themselves in the peculiar mode of vibration, rotation, or both combined, which we call magnetic; and this state of excitation is indefinite in its continuance. This is stating the case generally as it appears to the writer; but if we particularize, the discrepancy will appear the greater: viz., a piece of steel is passed over another which is already in the magnetic state; and if this is done inductively,

* Communicated by the Author.

the primary magnet loses nothing of its own power; therefore the only apparent force to which we can look for the production of the newborn magnetism is that exerted in moving the piece of steel through the lines of magnetic force of the parent magnet. To the writer it seems impossible to believe that the temporary exertion of muscular or other energy is thus converted into a force which is capable of indefinite multiplication by its own exertion through time. He feels that such a conclusion could not be accepted in the case of other phenomena. The force used to squeeze out the air from between two perfectly straight polished surfaces is not now accepted as the cause of their continued adhesion, as the muscular exertion used to invert the barometer-tube is not the measure of the force which holds so many ounces or pounds of mercury *apparently* against the gravitating force. We know that the very force which appears to be militated against is the maintaining power; and the analogy which to the mind of the writer is presented in some of the phenomena of magnetism leads him to expect that, when its sustaining-power is fully understood, it will be found to be the same force as that which it exerts, and by which it is created, viz. *diamagnetism*. In putting forth these hypotheses, the writer does not so much intend to direct the minds of scientific superiors as to the definite end of a research into magnetic and diamagnetic action as to give a sort of explanation of the drift of the following experiments, which might otherwise not be understood from his point of view.

Diamagnetism is polar and dual. Static electricity gives the opportunity of splitting this duality and separating the polarity, by the electrification of matter in the positive and negative states respectively. He trusts he is not too bold in making these assertions; but philosophers have so long been accustomed to take up the argument from the other end, that, although not dealing with this hypothesis in the present paper, he is compelled to risk agreement with the thoughts of others.

A magnet is a circuit of power, the lines of *expression* of force equal in amount within and without; those within have a relation in direction to those without, and in fact are continuations of them absolutely unchanged in their nature, so far as the experimental test can be applied to them. Every line of force, therefore, at whatever distance it may be taken from the magnet, must be considered as a closed circuit, passing in some part of its course through the magnet, and having an equal amount of force in every part of its course*. If, then, we can cut off the lines of force exterior to the magnet, or any part of them, the total magnetic force exerted should be disturbed to the extent

* Faraday, Experimental Researches, Series XXVIII. 3117.

of the interference. In other words, if magnetism be dependent on the diamagnetic polarity of the medium surrounding it, any alteration of, or interference with that polarity should likewise be shown in a corresponding alteration of the magnetic force itself.

A fine darning-needle magnetized to saturation, suspended equatorially by a filament of silk or a fine hair so that it was free to swing horizontally, was the test-needle used in the following experiments. The suspended needle being come to rest, when either the north or south pole was approached by an electrified glass tube, it was attracted by it, and could be made to follow the tube round and round; or if the tube were laid down at such a convenient distance as prevented actual contact, but in the same direction as the magnetic meridian, the needle then set itself perpendicular to the electrified tube, gradually returning to its normal position as the tube's electricity became dissipated through the air or otherwise.

A stick of sealing-wax or other negative dielectric gave the like result; or by approaching both cautiously at the same time, the needle was maintained without alteration of position.

The needle was then itself electrified by contact; and whether positive or negative electricity were employed, its behaviour was apparently identical with that of a piece of copper wire suspended by a torsion-thread under similar circumstances, being repelled by like, and attracted by unlike or neutral electrics—returning, however, to the magnetic meridian as the electricity became dispersed.

A horseshoe-magnet with a lifting-power of about three pounds, and which had not been remagnetized for some years, so that its magnetic force might be reckoned as constant, was placed on a level with the suspended test-needle, and at just such a distance that actual contact could not take place betwixt the opposing poles. Of course the needle immediately took up its position with its north pole opposite the south, and the south opposite the north pole of the magnet, the sharp suddenness of its vibrations in coming to rest testifying to the inductive power mutually exerted. A body electrically excited being then presented to it, attraction was immediately manifested; the needle could be drawn bodily away from the magnetic field; or if the separate ends were acted on, the north or south pole could be drawn completely round till it was opposite the pole of the magnet of the same name; or if the needle was itself positively electrified, then repulsion was manifested for positively, and attraction for negatively electrified bodies; and the opposite behaviour if electrified negatively.

Similar results were likewise obtained when very small quantities of static electricity were employed; a piece of sealing-wax

half an inch in length sufficed to give an amount of electricity energetic enough to modify materially the forces exerted by the magnets in their mutual relations, though not sufficient to absolutely neutralize for the time their respective polarity.

The experiment was then reversed: an electrified glass tube was presented to the north end of the test-needle in such a manner that it was drawn across the magnetic meridian; and whilst so held a small bar-magnet, held horizontally and parallel to the tube, was gradually brought near. Under these circumstances the needle, when the bar-magnet was close enough, could be brought to a state of equilibrium between the two, attracted and repelled by neither, but quite easily affected by an electrified body, another magnet, or a piece of soft iron being approached to it. The same state of equilibrium was obtained by presenting to either pole of the needle the pole of the magnet of the same name—in the one case placing it in juxtaposition with the tube, in the other opposite to it.

When the test-needle is electrified either positively or negatively, it is attracted by neutral conductors; but if conductors round about it be at something like equal distances, its direction is then assimilated to the magnetic north and south.

The magnetic needle, in an apparently neutral state between the opposing magnet and electrified body, was examined and found to have the opposite static electricity to that acting inductively on it; so that it would appear capable of holding the two forces at the same time.

The foregoing experiments appear to the writer to demonstrate that:

If the one or the other or both poles of a magnet are subject to the inductive influence of another magnet, the effect of bringing static electricity into the magnetic field, whether it be in the positive or negative state of separation, is to weaken their mutual inductive power.

A piece of soft iron wire suspended by the middle behaved in the magnetic field as if it were a magnet when acted on by the opposing forces, the mutual reaction being very clearly seen when the magnetic or electric force was in preponderance or closer approximation; and if the two were made to unite in direction, the iron was capable of being drawn to either the electrifier or the magnet from a greater distance than it could have by either *per se*. A strongly electrified glass tube placed at right angles to the poles of the horseshoe-magnet before mentioned, whilst the soft iron was suspended in front, charged the end of the iron nearest to itself with positive, the further end with negative electricity; and the electric attraction for a neutral mass, added to the magnetic attraction, enabled the magnet to

attract it at a greater distance than it previously did. The wire was pointed.

The writer next insulated the horseshoe-magnet, and endeavoured to make evident a reduction of magnetic attractive power when it was itself charged with electricity.

Pieces of soft iron wire one and a half inch in length were attached by attraction successively to one pole till the point at which the magnet ceased to attract was nearly reached. Electricity was then passed into the magnet; and during the charging process the last piece of wire in the magnetic chain first vibrated strongly, and finally fell off. When two pieces of iron wire were hung side by side at the end of the chain, their divergence was great whilst the magnet was receiving electricity; and if the attracting force was feeble, one or the other of the pieces dropped off.

Another mode in which the experiment was performed was by elongating either pole with a piece of soft iron two inches long and held by magnetic attraction, to the end of this, again, pieces of soft iron wire about one inch in length, and then partially closing the magnetic circuit by a stout nail laid across the limbs of the magnet: by moving the latter nearer to or further from the poles the force holding the wires at the end of the elongated pole could be brought within limits that enabled the feeble electricity employed to contend with it. Under these circumstances, whenever the magnet was charged with electricity, one or more wires immediately fell off.

After repeating the experiment a few times with the same wires, the same ends being attached to the same pole each time, it was found that they had become to a slight extent magnetic, the magnetism according in polarity with the law of magnetization by contact; so that the electricity had to overcome an additional attraction, which it probably itself assisted to create, in addition to that previously existing. A powerful electrical machine would probably enable an experimenter to demonstrate these effects more fully.

He then placed the magnet on the upper side of a glass plate, the lower side of which, except for about two inches round the edges, was coated with tinfoil, and had a good earth connexion, and, throwing some fine iron-filings on the poles, charged the system. Whilst doing this a number of filings were thrown off from the poles and dispersed, the filaments of adhering particles first rapidly vibrating and oscillating, each filament exercising a greater degree of repulsion on its neighbour than it previously did, and finally jumping off in small masses, which instantly dissipated as soon as they got beyond the scope of the magnet's greatest intensity. On making contact between the tinfoil and magnet, the filaments returned to their original degree of diver-

gence. On again charging the magnet another portion of filings was thrown off; and the writer in this way succeeded in collecting in front of the magnet about one-twentieth part of the filings employed, after which he was unable further to separate them than by increasing the divergence of the iron filaments on the magnetic poles each time the arrangement was charged.

The filings were removed, and the test-needle brought over the magnet so that the point was drawn downwards to an angle of 60° in its endeavour to attain contact with the opposing pole, but was not allowed quite to reach it. The magnet was then charged as before; and at once the needle began to withdraw itself, the point becoming charged with positive, the head with negative electricity; a conductor was presented to it; it discharged itself and returned towards the magnet, and could have been kept in this state of oscillation indefinitely. When the magnet was discharged, the needle at once flew back to its former position; left to itself, the point by degrees discharged itself into the air and returned to the magnet.

Two needles just touching the poles of the magnet, in the arrangement above described, could not be repelled; but when the magnet was charged, their free ends took a much greater degree of divergence than they previously had.

Albemarle Street.

[To be continued.]

XII. Notices respecting New Books.

Introduction to the Study of Inorganic Chemistry. By WILLIAM ALLEN MILLER, M.D., D.C.L., LL.D., &c. London: Longmans. 8vo. Pp. xi and 288.

A Cyclopædia of Quantitative Chemical Analysis. By FRANK H. STORER, A.M., Professor of General and Analytical Chemistry in the Massachusetts Institute of Technology.—Part I. Boston and Cambridge (U.S.): Sever. London: Spon. Large 8vo. Pp. iv, 112 and 8.

Handbook of the Metals. By THOMAS ALLEN BLYTH, M.A., Ph.D., F.E.I.S., F.G.S.E., University of Göttingen. London: Longmans. Small 8vo. Pp. ii and 128.

THE Messrs. Longmans are issuing a series of elementary works on various branches of mechanical and physical science. These are intended for general use in schools, and for the self-instruction of working men; to both of which purposes their uniformly low price and moderate amount of matter are decidedly appropriate. The late Dr. Miller, having undertaken the authorship of a chemical textbook for this series, had completed his task and was engaged in reading the proof-sheets, when he was seized with an illness which unhappily proved fatal. His friend Mr. C. Tomlinson, F.R.S., to whom

the book had already owed certain changes in style and arrangement, undertook to see it through the press, and has discharged his trust with the very great care and pains which distinguish his contributions to scientific literature.

Self-instruction in any subject is generally unsystematic in its course and vague in its results; few have recourse to it, unless to find that they have lost time and made habitual mistakes in the absence of a teacher. Yet, if we were asked to name the most satisfactory chemical handbook for a self-instructing student, the one now before us would naturally occur to our mind. It is so simple and straightforward. These were characteristics of the author himself; and they would have rendered it easy to recognize the work as his, even had no name been prefixed to its pages.

Dr. Miller's book is divided into seventeen chapters. The first relates to preliminary matters, such as the Scope and Aim of Chemistry, Chemical Notation, Mixture and Combination. The next nine chapters relate to the Non-Metals; the remainder to the Metals. Short explanations of theoretical points are scattered throughout the book; and very clear directions are given for the performance of a large number of experiments, which for the most part only require very little outlay for their execution. The metric system, the centigrade scale of temperature, and the Berzelian nomenclature are adopted throughout. Dr. Miller's text-book is altogether one of the most useful elementary manuals we have met with for a long time.

Professor Storer's 'Cyclopædia' has been compiled with the view of making quantitative chemical methods more accessible in their literary aspect than heretofore. Hence he prefers an alphabetical arrangement. Moreover, after he has named any substance that is intended to undergo determination, he prominently specifies the "principle" upon which the estimation is based, the "applications" of the principle, and then the "methods" of the estimation itself. Thus, under ANTIMONY we find "*Principle I.* Sparing solubility of the metal in chlorhydric acid. *Applications.* Estimation of antimony in antimony-salts. Separation of antimony from tin. *Method A.* Precipitate the antimony by means of metallic zinc from a dilute nitric solution. *Methods B, C, D, &c.*" In this manner it is easy for a working chemist to select at once, without the trouble of reading many paragraphs (as in other analytical treatises), the method that suits his particular problem for the time being. The various processes described in this first Part of the Cyclopædia are given with very considerable care, great directness of description, and copiousness of detail. We regret, however, the intentional absence of figures from Professor Storer's work, and hope he will reconsider his decision upon this point. Without some pictorial aid it is impossible to describe satisfactorily such apparatus as a combustion-furnace, or to do justice to even a few of the more valuable modes of determining carbonic acid; and the narrative—graphic as it undoubtedly is—becomes lifeless without the picture.

Dr. Blyth's 'Handbook of the Metals,' as it is termed on the cover, becomes "Metallography as a Separate Science" on the title-page.

It is dedicated to the President, Council, and Fellows of the Edinburgh Geological Society, and is intended for the use of schools and science-classes. "This treatise, consisting of a reprint of the author's contributions to various magazines, is issued merely as an elementary work on the science." Our own perusal of the book has led us to a different conclusion. It is really issued in order "to enlarge our ideas of the wisdom and power of the Divine Being, and, consequently, to exalt the mind." The manner in which this twofold object is sought to be effected is as follows:—A small allowance of metallic chemistry is put prominently forward, bald, inaccurate, incomplete, and utterly destitute of the slightest logical sequence. Throughout the dreary desert of description, oases of poetry are cunningly planted, so as to lead the reader to the refreshing springs of natural theology, apparently of his own accord. Here, however, he will find thicket after thicket of Scripture texts somewhat incongruously flourishing; and may not improbably interpret them as evidence of design, though not very honest design. Recurring to the poetry, he will remember that it consists of a few solemn pieces from really great masters, and many pieces that are mere rubbish (some of the latter are significantly anonymous); Eulalia Gurson rhymes eloquently about the action of potassium upon ice, but unhappily has no respect for the laws of accent. And he will probably agree with us that our ideas are not to be enlarged after such a fashion, nor our minds exalted.

We have seldom seen so much titular distinction united to so reprehensible a performance.

Text-Books of Science. The Elements of Algebra and Trigonometry. By W. N. GRIFFIN, B.D., some time Fellow of St. John's College, Cambridge. Longmans and Co. 1871. Pp. 312.

This book consists of three parts. The first contains the usual course of algebra up to and including quadratic equations; the second contains a rather full account of the properties and uses of the logarithms of numbers; the third contains the solution of plane triangles, the ordinary questions of heights and distances, a few questions of mensuration, with just so much of the theory of trigonometry as is necessary as a foundation for these applications. It follows, of course, that the treatment of the subject of trigonometry is far from complete; *e. g.* the whole subject of the relations between the goniometrical functions of angles is omitted. On the other hand, within the prescribed limits, the subjects are treated very fully; and while the explanations and expositions are amply sufficient for the learner, they are not excessive. The book contains a very large number of well-chosen examples; this, in such a book, is a capital point; and there is no doubt that the student who works conscientiously through the book will obtain a thorough knowledge of the subjects which it professes to teach.

We should like to know what is Mr. Griffin's authority for the following statement:—"In France and other parts of the continent it is usual to measure angles by a decimal division. A right angle

being made, as with us, the starting-point, the hundredth part of it is called a grade &c." (p. 218). We are, of course, aware that what is called the French division of the circle is given in most English text-books, but had always thought it appeared in them mainly on grounds of conservatism, or because examiners sometimes ask for an account of it. The history of the matter is thus stated (and, we believe, correctly) by the late Dean Peacock:—"The French, simultaneously with the establishment of their *système métrique décimale*, proposed to divide the quadrant into 100 degrees, the centesimal degree into 100 minutes, and the centesimal minute into 100 seconds, and so on; and this division was adopted in the *Mécanique Céleste* of Laplace and other contemporary scientific works. The change, however, from the *nonagesimal* to the *centesimal* degree was attended with no advantage sufficient to compensate for the great sacrifice of tables and records which its adoption rendered necessary, and its use was speedily abandoned, even in France" (Treatise on Algebra, vol. ii. p. 146, note).

Algebraical Exercises and Problems; with Elliptical Solutions. By HUGH MCCOLL. Pp. 100. Longmans and Co. 1870.

This may be fairly called a remarkable book: in the well-worked field of elementary algebra the author has hit on some things which (so far as our knowledge goes) are new. The value of the novelties is, to be sure, a different question. In the first place the "elliptical solution" is a novelty. We shall best give a notion of what is meant by an elliptical solution by producing a specimen. The question is this:—"A plays at chess with B, winning 3 games out of 4; and afterwards with C, winning 2 games out of 3; at the end of 21 games he has won 15. How many did he play with each?" (p. 34). And this is the elliptical solution:—"He played x games, suppose, with B, out of which he won . . . games; and he played . . . games with C, out of which he won . . . games. And since we are told that he won altogether . . . games, we have the equation

$$\dots = \dots$$

from which we get $x=12$ " (p. 69). The elliptical solutions give just the kind of information which a teacher commonly gives *vivâ voce* to the learner who has not yet acquired the art of expressing questions in an algebraical form, and there can be no doubt as to their value.

The second novelty is this:—A considerable number of examples are set in a form such as the following. "If $1 - \frac{1-x^2}{1+x^2}$ ounces cost $1 - \frac{1+x^2}{(1+x)^2}$ shillings, what is the price per ounce? What would $x + \frac{1}{x}$ ounces cost at the same rate?" (p. 27). This is, of course, equivalent to the exercise commonly written thus:—Simplify

$$\left(x + \frac{1}{x}\right) \left(1 - \frac{1+x^2}{(1+x)^2}\right) \div \left(1 - \frac{1-x^2}{1+x^2}\right).$$

We very much question whether in practical teaching the new form has any advantage over the old. In an appendix Mr. McColl proposes a uniform method of resolving algebraical expressions into factors which can frequently be employed with advantage. On the whole this is a book from which any one engaged in teaching algebra may get useful hints, though we do not suppose it will displace any of the many excellent elementary works on Algebra at present in common use.

XLII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 241.]

Dec. 22, 1870.—General Sir Edward Sabine, K.C.B., President, in the Chair.

THE following communications were read :—

“On the Extension of the Coal-fields beneath the Newer Formations of England; and the Succession of Physical Changes whereby the Coal-measures have been reduced to their present dimensions.” By Edward Hull, M.A., F.R.S., F.G.S., Director of the Geological Survey of Ireland.

In this paper the author, embodying with his own the observations of previous writers on the physical geology of Great Britain, especially those of Murchison, Godwin-Austen, Ramsay, Phillips, and the late Professor Jukes, showed that the Coal-measures were originally distributed over large tracts of England, to the north and to the south of a central ridge or barrier of Old Silurian and Cambrian rocks, which stretched across the country from North Wales and Shropshire into the Eastern Counties, skirting the southern margin of the South Staffordshire Coal-field. This barrier, or ridge, was a land-surface till the close of the Carboniferous period.

To the north of the central barrier, the highlands of Wales, the mountains of the Lake-district, and probably small tracts of the southern uplands of Scotland formed land-surfaces skirting portions of the Carboniferous area, while the Carboniferous tract to the south of the central barrier was probably bounded by a land-surface trending along the southern coast of England. The distribution of the Coal-measures at the close of the Carboniferous period was illustrated by a Map, No. 1.

It was then shown that the whole Carboniferous area was subjected to disturbances through the agency of lateral forces, whereby the strata were thrown into folds along axes ranging (approximately) in east and west directions; and as denudation accompanied and followed these disturbances, and acted chiefly over the arches (or anticlinals), large tracts were divested of Upper Carboniferous strata, and thus the first phase in the marking out of the limits of our present coal-fields was brought about. The effects of these movements and denudations were illustrated by Map No. 2.

The disturbances which ensued after the deposition of the Permian

strata, and which produced the discordances of stratification between the newer Palæozoic and Mesozoic formations, were shown to have acted along lines ranging approximately north and south, parallel to the axis of the Pennine Chain, and consequently in a direction transverse to those of the previous period. These disturbances were also accompanied by the denudation of strata from the anticlinal arches, and the consequent disseverance of the Coal-measure tracts over certain definite areas. The results of these movements (the second phase in defining the bounds of the coal-fields) were illustrated by Map No. 3.

From a consideration of the foregoing observations, the author came to the conclusion that the tendency of the British coal-fields to arrange themselves into the form of "basins" (sometimes partially concealed by newer strata), a tendency strongly insisted on by Prof. Ramsay, was due to the intersection of the two systems of flexures above described, one anterior to the Permian period, the other anterior to the Triassic period, and that the actual disseverance of the coal-fields into basins was due to denudation acting with greatest effect along the anticlinal arches of these flexures.

The inference that the Yorkshire and Durham coal-fields are really basins rising to the eastward under the Mesozoic strata was drawn, an inference supported by the easterly rise of the Coal-measures along the sea-coast from the Coquet to the Tyne.

Guided by these principles, the author maintained that we are now in a position to determine with great accuracy the actual limits of the Coal-measures under the Mesozoic formations over the area to the north of the central barrier ridge (as indicated on Map No. 3), and that to the south of the ridge the application of the same principles would assist towards the solution of the question, though in a less degree, owing to the fewer opportunities for observation of the Palæozoic formations.

The author, however, concurred in the views advanced by Sir R. I. Murchison*, that, in consequence of the great amount of denudation which the Carboniferous rocks had undergone over the area of the south of England previous to the deposition of the Mesozoic formations, little coal was to be expected to remain under the Cretaceous rocks.

"On the Constitution of the Solid Crust of the Earth." By the Ven. John Henry Pratt, Archdeacon of Calcutta, M.A., F.R.S.

In this paper the author applies the data furnished by the pendulum-observations recently made in India to test the truth of the following hypothesis regarding the Constitution of the Earth's Crust, which he propounded in 1864, viz.:—that the variety we see in the elevation and depression of the earth's surface in mountains and plains and ocean-beds has arisen from the mass having contracted unequally

* In his Address at the Meeting of the British Association at Nottingham, 1866. On the other hand, the views of Mr. R. Godwin-Austen, which tend rather in an opposite direction, should be well weighed by all who are interested in this question. (Quart. Journ. Geol. Soc. vol. xii.)

in becoming solid from a fluid state ; and that below the sea-level, under mountains and plains, there is a deficiency of matter approximately equal in amount to the mass above the sea-level ; and that below ocean-beds there is an excess of matter approximately equal to the deficiency in the ocean when compared with rock ; so that the amount of matter in any vertical column drawn from the surface to a level surface below the crust is now, and ever has been, approximately the same in every part of the earth.

In order to make this hypothesis the subject of calculation, the author takes the case of the attenuation of matter in the crust below mountains and plains, and the excess of matter below ocean-beds, to be *uniform* to a depth m times the height above the sea-level or the depth of the ocean, as the case may be.

The results are shown in the following Table, in which the numbers are the last figures in the ratio of the differences of gravity to gravity itself, carried to seven places of decimals. The decimal point and ciphers are omitted for convenience.

Stations.	Differences of gravity.			
	Relative effects of local attraction deduced from pendulum-observations.	Residual errors after correction by the method of		
		Dr. Young.	This hypothesis.	
			<i>m</i> = 50.	<i>m</i> = 109.
<i>Indian-arc stations.</i>				
Punnoe
Bangalore	+384	-562	- 78	-557
Damargida	-323	-926	-455	-584
Kalianpur	+341	-208	+338	+315
Kaliana	-707	-957	+ 69	+320
<i>Coast stations.</i>				
Punnoe
Alleppy	+302	+314	+331	+360
Mangalore	-166	-154	-122	- 79
Madras	-197	-192	-138	- 78
Cocanada	+142	+153	+216	+291
<i>Ocean station.</i>				
Minicoy Island	+894	+906	+ 31	+102

The author points out from this Table that Dr. Young's, or the usual method of correction for local attraction, so far from improving matters, introduces very large residual errors of the arc and ocean stations, and, at places on the arc of meridian, all lying on the same side with reference to Punnoe. He observes that neither the usual method nor his own much affects the coast stations ; and attributes this to the want of more complete knowledge of the contour of the surface, both above and below the sea-level, in these parts. But his own method, in the case $m=50$, remarkably reduces the effects of

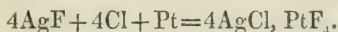
local attraction at stations on the arc of meridian and out at sea (in Minicoy, an island 250 miles west of Cape Comorin or Punnoe); for the sensible negative quantity at Damargida and positive quantity at Kalianpur indicate a deficiency of matter below the first and an excess below the second, which exactly tally with the results independently brought out by relative deflections of the plumb-line as obtained by the survey: and the two large and most important effects, negative at Kaliana and positive at Minicoy, may be said to be almost annihilated by this method of correction. This last case of an *excess* of gravity out at sea (where the surrounding ocean has a deficiency of matter) being explained by his method he regards as a very strong argument in its favour. And he finishes by saying that if his method is thus far successful in the particular supposition of the distribution below, whether in excess or defect, being *uniform*, which is most likely not strictly the case, there is every reason for concluding that pendulum-observations give support to the hypothesis regarding the Constitution of the Earth's Crust, when viewed on a large scale, admitting of local peculiarities, like the deficiency of matter near Damargida and the excess near Kalianpur, and the similar deficiency near Moscow.

January 12, 1871.—General Sir Edward Sabine, K.C.B., President, in the Chair.

The following communications were read:—

“On Fluoride of Silver.”—Part II. By George Gore, F.R.S.

This paper contains an exhaustive account of the behaviour of argentic fluoride in vessels of platinum, carbon, and various fluorides in contact with chlorine, bromine, and iodine at various temperatures. When argentic fluoride is completely decomposed by chlorine in platinum vessels at a red heat, the reaction agrees with the following equation:

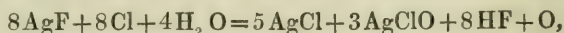


Vessels of cryolite and of fluor-spar were found incapable of retaining argentic fluoride in a melted state. Other vessels were also made by melting and casting various mixtures of earthy fluorides at a high temperature; and although forming beautiful products, probably capable of technical uses, they were not capable of retaining silver fluoride in a state of fusion. Numerous vessels were also made of seventeen different fluorides by moulding them in the state of clay and baking them at suitable temperatures; these also were found incapable of holding melted fluoride of silver. Argentic fluoride was only superficially decomposed by chlorine at 60° Fahr. during thirty-eight days. When heated to 230° Fahr. during fifteen days in a platinum vessel in chlorine, it was very little decomposed. *Chloride* of silver heated to fusion in a platinum vessel in chlorine corroded the vessel and formed a platinum-salt, as when fluoride of silver was employed.

An aqueous solution of argentic fluoride agitated with chlorine evolved heat and set free oxygen, in accordance with the following

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equation:—



or



Dry hydrochloric acid gas completely decomposed argentic fluoride in a melted state, but only acted upon it superficially at 60° Fahr. A saturated aqueous solution of argentic fluoride was not precipitated by chloric acid.

Perfectly anhydrous fluoride of silver was only superficially decomposed by contact with bromine in a platinum vessel during thirty-six days at 60° Fahr., or during two days at 200° Fahr. At a low red heat in vessels of platinum, argentic fluoride was completely decomposed by a current of bromine vapour, a portion of its fluorine being expelled and a portion corroding the platinum and forming an insoluble compound of fluoride of platinum and bromide of silver. In carbon boats at the same temperature the whole of the silver-salt was converted into bromide, the boat being corroded and the fluorine escaping in chemical union with the carbon. The action of bromine on an aqueous solution of argentic fluoride was similar to the action of chlorine. A solution of argentic fluoride yielded copious precipitates both with hydrobromic and bromic acids.

Under the influence of a temperature of 200° to 600° Fahr. in closed platinum vessels, iodine very slowly and incompletely decomposes argentic fluoride without corroding the vessels, and produces a feeble compound of argentic iodide, fluorine, and iodine, from which the two latter substances are expelled at a red heat. At a red heat in platinum vessels, iodine produces argentic iodide, and in the presence of free argentic fluoride corrodes the vessels in consequence of formation of platinic fluoride; iodine and fluorine pass away together during the reaction. In vessels of carbon at the same temperature argentic iodide is formed, the vessels are corroded, and a gaseous compound of fluorine and carbon is produced. By treating an aqueous solution of argentic fluoride with iodine, similar results are produced as with bromine and chlorine; a similar solution yields copious precipitates both with hydriodic and iodic acids.

A mode of analysis of iodine is also fully described in the paper. A known weight of iodine was dissolved in absolute alcohol, a strong solution of argentic nitrate of known strength added to it, in portions at a time, with stirring until the colour of iodine exactly disappeared. The mixture was evaporated, the free nitric acid expelled by careful heat, and the residue weighed. The residue was then heated to fusion, to convert the iodate of silver into iodide, and again weighed. Two experiments of this kind yielded accurate results; and the process was easy and expeditious.

“Polarization of Metallic Surfaces in Aqueous Solutions, a new Method of obtaining Electricity from Mechanical Force, and certain relations between Electrostatic Induction and the Decomposition of Water.” By Cromwell Fleetwood Varley.

Platinum plates immersed in sulphuric acid and water, as in a de-

composition-cell, require a potential of about 1.7 Daniell's cell to decompose the water; with potentials of less amount the platinum plates can be charged and discharged like condensers. They have enormous electrostatic capacity. Mercurial surfaces equally admit of polarization with hydrogen. A surface of mercury in dilute sulphuric acid, when made negative to the water by means of a powerful battery, flattens out. If the mercury be replaced by an amalgam of proper consistency, the flattening out is increased; the reversal of the current restores the amalgam to its original dimensions. By reversing the process, electric currents can be obtained from mechanical force.

A large vessel on a board has within it two shallow funnels, which are connected by means of glass tubes with similar vessels outside of the large one. Pure mercury is poured into the funnels until they and the outside vessels are one-third filled. By tilting the board, mercury runs into the one funnel and out of the other, and thus the surface in the one is made to increase while that in the other decreases. Dilute sulphuric acid is poured into the larger vessel so as to cover the two funnels; the latter are connected together through a galvanometer.

If the mercury is pure and free from polarization, the tilting of the board produces no electric current. On polarizing one of the surfaces with hydrogen by a battery, it gives rise to a current through the galvanometer, and thus shares the polarization over the two surfaces. If the battery be removed, on augmenting the one surface and diminishing the other, a current of electricity is seen to pass through the galvanometer.

A convenient method of showing this experiment on a large scale is to procure a gutta-percha trough 4 inches deep and 4 by 2 inches broad. A partition of the same material 2 inches high divides the lower half into two separate chambers: these are partly filled with mercury; amalgamated platinum plates, hung from a balance-lever, dip into the mercury. On depressing one set of plates the others are elevated, and thus the mercurial surface exposed to the fluid is alternately augmented and diminished to a large amount. Twelve of these arranged in series give a current of rather more potential than one cell of Daniell's battery when the mercury is polarized with hydrogen. The addition of a minute fragment of zinc to the mercury maintains the polarization for a very long time, and the power is considerably increased thereby. When a large surface of mercury (25 circular inches) has been polarized with a power of half a Daniell's cell and is rapidly reduced to the diameter of $\frac{1}{30}$ inch by letting the mercury flow out of the funnel, some bubbles of hydrogen gas appear just as the last of the mercury is running out, the decrease of surface evidently augmenting the potential sufficiently to decompose the water: floating a small piece of platinum on the mercury renders this phenomenon much more distinct.

All attempts to polarize the mercury with oxygen have failed to give a current. By depolarizing the mercury with a battery until no

current is generated by varying the dimensions of the exposed mercury surface, a metallic surface neutral to the fluid is obtained.

The second part of the paper refers to the electrostatic capacity of platinum plates in dilute acid and water.

In order to determine this point, it is necessary to use sensitive, rapidly oscillating, reflecting galvanometers of very small resistance. The author has succeeded in measuring the charge which a square inch of platinum exposed to another square inch of platinum surface receives from potentials varying from 0.02 of a Daniell's cell up to 1.6 Daniell's cell. From a potential of 0.02 to 0.08 the capacity remains sensibly constant; that is, the discharge from the plates varies directly as the potential. When the potential increases beyond 0.08, the charge which the plates receive increases in a greater ratio, the capacity being 3.3 (in one experiment) and 3.1 (in another experiment) times as great with a potential of 1.6 as it was with the potential of 0.1.

There is great difficulty attending accurate determination of the latter amounts; but the author expects that this increase of capacity will be found to vary as the square root of the potential. The capacity of the platinum plates with varying powers is shown in the accompanying Tables.

The author thinks these experiments tend to show that the fluid does not actually touch the platinum plate, but is separated from it by a film, which film, if a pure gas, must be less than the $\frac{1}{1,000,000,000}$ part of an inch, when very small potentials are used. This distance decreases as the potential rises. Inasmuch as two surfaces equally electrified with the opposite electricities attract each other with a power varying inversely as the square of the distance, the experiment would seem to indicate that at very small distances the platinum repels the water with a power varying inversely as the cube of the distance.

The phenomena of electrification render accurate determinations of the capacity extremely difficult. The fact of the phenomena of electrification being present leads the author to think that the separating film (if such a film exists) is not a pure gas, but has five or more times as much electrostatic capacity as pure gas.

A useful inference drawn from the above experiments is the impossibility of working through any considerable length of uninsulated wire in the ocean.

The French Atlantic cable from Brest to St. Pierre works, upon the average, ten words per minute; the author calculates that a solid conductor of the same weight per mile as that used between the above stations must be reduced to a length of less than 1100 yards in order that the rate of signalling through it shall be not slower than through 2500 miles of the same conductor insulated; and the bare wires can only be practically worked on circuits not exceeding a mile.

TABLE I.

Two platinum bulbs about 0.75 inch in diameter in dilute sulphuric acid.

Owing to the large resistance (1000 Ohms) used in R and R', the actual potential is uncertain in this experiment, because the conduction across the fluid reduces it.

1. Potential, in terms of a cell of Daniell's battery.	2. Duration of electrifi- cation, in seconds.	3. Swing of reflecting galvanometer by the discharge of the bulbs on raising the key.				4. Current after magnet came to rest.	5. Mean minus the remaining current.	6. Mean divided by potential and 100 to give relative capacity for various potentials.	7. Approximate capa- city, in micro- farads.
0.02	10	21½	21½	21½	21½	½	2	1	348
"	20	21½	21½	21½	21½	"	"	"	"
0.04	10	4½	4½	4½	4½	"	4	1	"
0.06	10	6½	6½	6½	6½	"	6	1	"
0.1	10	11½	11½	11½	11½	1	10½	1.05	365
"	20	11	12	11½	11½	"	"	"	"
0.16	10	18	17½	18	18	1¼	16¾	1.09	379
"	20	18	18	18	18	"	"	"	"
0.2	10	24	24	24	24	1½	22½	1.12	390
"	20	24	24	24	24	"	"	"	"
"	30	24	24	24	24	"	"	"	"
0.4	10	59	58	58	58	2½	55½	1.39	484
"	20	58	57	58	58	2½	"	"	"
"	30	58	58	57	57	"	"	"	"
0.6	10	105	105½	104	104	"	"	"	"
"	20	105	104	105	105	"	104½	1.74	606
"	30	104	105	103	103	"	"	"	"
0.8	10	164	163	162	162	3	159	1.99	693
"	20	162	162	161	161	"	"	"	"
"	30	162	162	161	161	"	"	"	"
1.0	10	230	235	230	230	5	226	2.26	786
"	20	232	231	230	231	"	"	"	"
"	30	231	231			"	"	"	"
*1.2	30	318	320	314		14	303	2.53	880
*1.4	30	440	446	451		23	426	3.04	1057
*1.6	30	about 603				30, 41, 52	562	3.5	1218

* Last three readings doubtful, the current remaining after the discharge being considerable. The true readings would be greater than those indicated.

Condenser of 311 microfarads.

0.02	...	1½	1½	1½	1½	1.03	311
0.04	...	3	3	3	3	"	"
0.06	...	4½	4½	4½	4½	"	"
0.08	...	6	6	6	6	"	"
0.10	...	7½	7½	7½	7½	"	"
0.2	...	14½	14½	14½	14½	1	"
0.4	...	29	29	28½	29	"	"
0.6	...	43	43½	44	43½	"	"
0.8	...	58	58	58	58	"	"
1.0	...	72½	73	73	73	"	"
1.6	...	116	116	116	116	"	"
2.0	...	143	143	143	143	"	"

The condenser of 311 microfarads capacity consisted of 24,300 square feet of metal surface insulated by thin paper and paraffin wax.

TABLE II.

Two platinum plates in acid and water, each exposing 1 square inch surface. The resistance of $R + R' = 100$ Ohms in this Table; by experiment the potential of the two cells was found to be reduced 8 per cent., and was therefore very nearly 200 lbs. instead of two Daniell's cells.

Approximate potential, in volts.	Time of electrification.	Throw of image by discharge of plates.				Current remaining after discharge.	Mean minus the current.	Ratio of capacity with different potentials.	Value in microfarads.
	seconds.								
0.2	10	19	20	19		1	18	1	175
"	20	18	19	19		"			
0.4	10	45	46	46		3	43	1.2	210
"	20	46	46			"			
0.8	10	175	170	170	165	11	159	2.2	385
1.0	10	230	228	226		18	210	2.33	408
*1.2	10	310	308	311		22	288	2.67	467
*1.4	10	373	380	382		30	350	2.77	484
*1.6	10	460	460	467	475	33	428	3.10	542
Condenser of 311 microfarads.									
0.2	...	32	32	32		0	32	1	311
0.4	...	63	64	63½		"	63½	"	"
0.8	...	127	127			"	127	"	"
1.0	...	159	159			"	159	"	"
1.2	...	188	187	189		"	188	"	"
1.4	...	220	220	221		"	220	"	"
1.6	...	252	254	252	254	"	253	"	"
1.8	...	284	283	284		"	284	"	"
2.0	..	316	317	317		"	317	"	"

* These readings are uncertain, it being conjectural how much current remained after the image had swung out and back, its momentum lasting longer than with smaller deflections; the true readings would therefore be greater than those observed.

Jan. 19.—General Sir Edward Sabine, K.C.B., President, in the Chair.

The following communications were read:—

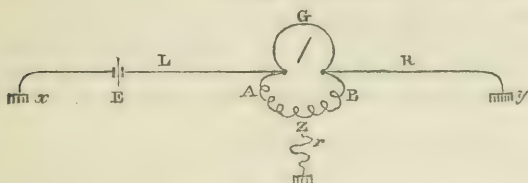
"Method of Measuring the Resistance of a Conductor or of a Battery, or of a Telegraph-line influenced by unknown Earth-currents, from a single Deflection of a Galvanometer of unknown Resistance." By Henry Mance, Superintendent Mekran Coast and Persian Gulf Telegraph Department, Kurrachee.

The resistance of each part of a circuit, such as that shown in fig. 1, being known, the influence exercised by the shunt A B, as well as the total resistance of the whole between x and y , can be easily ascertained by simple and well-known formulæ.

But let a leakage r , which we will suppose gives perfect earth, be applied at some point in the shunt A B, the deflection previously

produced on G by a current arising in L will probably be considerably changed. I say probably, because, by sliding the leakage r along

Fig. 1.



the whole length of the shunt, we shall at last find a point Z at which the needle will return to its original deflection; the position of Z being ascertained, any resistance, varying from infinity to "dead earth," may be applied without causing any change in the deflection of the needle.

It is evident that, although the total resistance of the circuit between x and y has been lessened by the insertion of the leakage, a proportionately larger amount of current is diverted from the galvanometer by that part of the shunt between L and the leakage at Z.

Presuming the electromotive E in L to remain constant, and taking $r=0$, we have the intensity of the current passing through G represented by the equation

$$\frac{E}{\left\{ L + \frac{G \cdot (A+B)}{G+(A+B)} + R \right\}} \cdot \left\{ \frac{G+(A+B)}{A+B} \right\};$$

but after r is connected, the equation becomes

$$\frac{E}{\left\{ L + \frac{\left(G + \frac{RB}{R+B} \right) A}{A + G + \frac{RB}{R+B}} \right\}} \cdot \frac{G + \frac{RB}{R+B} + A}{A}$$

As the condition that the galvanometer-deflection remains unchanged, the first of these equations must be equal to the second, from which we obtain the formula

$$L = R \cdot \frac{A}{B},$$

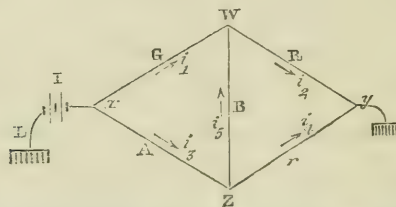
the resistance G being immaterial. It will therefore be seen that R always bears the same proportion to L that B does to A, the latter branches bearing some analogy to the proportion-coils of a Wheatstone testing-bridge.

Under certain circumstances a test might be taken without any battery at all. In a submerged cable there is frequently sufficient earth-current to supply the electromotive force in the branch L; if not, a small battery can be inserted to maintain a steady current, and the internal resistance of the cells afterwards deducted. The polari-

zation-current from a leakage of low resistance in a cable would enable us to find the resistance from either side through the fault without the application of a battery. And, lastly, this method may be used to ascertain the internal resistance of a battery.

The above method occurred to me about two years since, during some experiments made to determine the resistance of the bridge-circuit, and the exact proportion of current traversing each branch of the Wheatstone balance, when the potentials at W and Z are unequal.

Fig. 2.



If I equals the intensity of the current at x or y , and i_1, i_2, i_3, i_4, i_5 the intensities in the sections G, R, A, r, B , then

$$\frac{G \cdot (B + R + r) + BR}{A \cdot (B + R + r) + Br} + 1 = \frac{I}{i_1} \dots \dots \dots (1)$$

$$\frac{A \cdot (B + R + r) + Br}{G \cdot (B + R + r) + BR} + 1 = \frac{I}{i_3} \dots \dots \dots (2)$$

$$\frac{R \cdot (A + B + G) + BG}{r \cdot (A + B + G) + AB} + 1 = \frac{I}{i_2} \dots \dots \dots (3)$$

$$\frac{r \cdot (A + B + G) + BA}{R \cdot (A + B + G) + BG} + 1 = \frac{I}{i_4} \dots \dots \dots (4)$$

$$\frac{B \cdot (R + r) + (B + R + r) \cdot (A + G)}{Gr - AR} = \frac{I}{i_5} \dots \dots \dots (5)$$

Or if the current in the branch B passes from W to Z ,

$$AR - Gr$$

should be substituted for the denominator of the last equation.

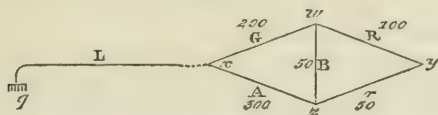
Equations (1), (2), (3), and (4) give the shunt-coefficient of the respective branches A, G, r, R ; thus, if G were a galvanometer, the strength of the deflection recorded multiplied by equation (1) would give the value of intensity I .

If, then, we consider G a galvanometer and the resistance r a leakage applied at Z , we have a similar diagram to that given in fig. 1; and the first of the five equations given above will enable us to determine the shunt-coefficient for the part A which lies between L and the leakage at Z .

Now this, together with the plan of testing described in the first paragraph, suggests an easy method for ascertaining by calculation the combined resistance of any system of derived circuits connected in the form of the Wheatstone's parallelogram; thus, if I wish to

know the resistance offered to the passage of a current between x and y in fig. 3, I can find it in the following manner.

Fig. 3.



First assume the existence at x of a sixth branch bearing (in resistance) the same proportion to R that A does to B ; that is to say, the supposititious branch

$$L = R \cdot \frac{A}{B}.$$

Now disconnect r from the point Z , and we have again a diagram similar to that in fig. 1; and as we have provided that $\frac{A}{B} = \frac{L}{R}$, the connexion or disconnexion of r at the point Z will make no difference whatever in the quantity of current passing from L into the branch G . I may therefore assume that, although the total resistance of the circuit between q and y has been decreased, the branch A has at the same time been able to divert a proportionately greater amount of current from the side G , in which the intensity remains unaltered.

If, then,

R_1 equals the resistance between q and y when the branch r is disconnected,

S_1 the shunt-coefficient of AB , which forms a shunt in the absence of r ,

R_2 the resistance between q and y after r is connected at Z ,

S_2 the shunt-coefficient for the part A , ascertained by equation (1), we have

$$R_1 \times S_1 = R_2 \times S_2,$$

$$R_2 = \frac{R_1 S_1}{S_2};$$

and R_2 minus the supposititious branch $\left(\frac{RA}{B}\right)$ will give the required combined resistance of the circuit between x and y .

Let R_3 be the combined resistance. Commencing with the equation

$$\frac{\left\{ \frac{RA}{B} + \frac{G \cdot (A+B)}{G+(A+B)} + R \right\} \times \frac{A+B+G}{A+B} - \frac{RA}{B}}{\frac{G \cdot (B+R+r)+BR}{A \cdot (B+R+r)+Br} + 1} = R_3,$$

we obtain

$$R_3 = \frac{\frac{R \cdot (A+B+G)}{B} + G}{\frac{G \cdot (B+R+r)+BR}{A \cdot (B+R+r)+Br} + 1} - \frac{RA}{B}.$$

If the potential at Z equalled that at W, the formula

$$R_3 = \frac{(G + R) \cdot (A + R)}{G + R + A + r}$$

would of course be sufficient.

“Measurement of the Internal Resistance of a Multiple Battery by adjusting the Galvanometer to Zero.” By Henry Mance.

The following method of taking the internal resistance of a battery will be found to give excellent results when several cells are to be tested.

Take one element from the rest of the cells, and arrange the circuit as in the annexed figure. Connect the poles of the battery under observation by a shunt S, and adjust the resistance of the latter till zero is obtained on the galvanometer.

Let E be the number of cells tested,

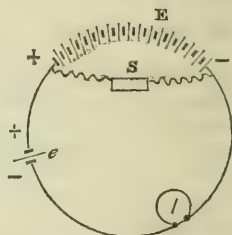
e number of cells opposed,

S = resistance of shunt,

R = internal resistance of E.

Then

$$R = S \frac{E}{e} - S.$$



In practice I usually returned the detached cell to the battery when $S \times E$ gave the internal resistance of the whole within a fraction of a unit.

It is assumed that the electromotive force in e equals that of the whole battery multiplied by $\frac{e}{E}$; the chance of error on account of this not being exactly the case would be lessened by detaching a larger number of cells than one, when the internal resistance of the remaining portion would be given by the first formula.

GEOLOGICAL SOCIETY.

[Continued from p. 155.]

Nov. 9, 1870.—Joseph Prestwich, Esq., F.R.S., President,
in the Chair.

The following communications were read:—

1. “On the Carboniferous Flora of Bear Island (lat. $74^{\circ} 30' N.$).
By Professor Oswald Heer, F.M.G.S.

The author described the sequence of the strata supposed to belong to the Carboniferous and Devonian series in Bear Island, and indicated that the plant-bearing beds occurred immediately below those which, from their fossil contents, were to be referred to the Mountain-limestone. He enumerated eighteen species of plants, and stated that these indicated a close approximation of the flora to

those of Tallowbridge and Kiltorkan in Ireland, the greywacke of the Vosges and the southern Black Forest, and the *Verneuilii*-shales of Aix and St. John's, New Brunswick. These concordant floras he considered to mark a peculiar set of beds, which he proposed to denominate the "Ursa-stage." The author remarked that the flora of Bear Island has nothing to do with any Devonian flora, and that consequently it and the other floras, which he regards as contemporaneous, must be referred to the Lower Carboniferous. Hence he argued that the line of separation between the Carboniferous and Devonian formations must be drawn below the yellow sandstones. The presence of fishes of Old-Red-Sandstone type in the overlying slates he regarded as furnishing no argument to invalidate this conclusion. The sandstones of Parry Island and Melville Island are also regarded by the author as belonging to the "Ursa-stage," which, by these additions, presents us with a flora of seventy-seven species of plants. The author remarked upon the singularity of plants of the same species having lived in regions so widely separated as to give them a range of $26\frac{1}{2}^{\circ}$ of latitude, and indicated the relations of such a luxuriant and abundant vegetation in high northern latitudes to necessary changes in climate and in the distribution of land and water.

2. "On the Evidence afforded by the Detrital beds without and within the North-eastern part of the Valley of the Weald as to the mode and date of the Denudation of that Valley." By S. V. Wood, Jun., Esq., F.G.S.

The author commenced by discussing the various hypotheses that have been proposed to explain the denudation of the Weald Valley. In his opinion the upheaval of the district took place in Postglacial times, and subsequently to the deposition of the gravels of the Thames Valley, of East Essex, and of the Canterbury heights; and the denudation was effected chiefly by tidal erosion during gradual upheaval in an inlet of the sea, aided by the action of fresh water flowing into this inlet from the north by streams draining the land which now constitutes the counties of Middlesex and Essex. The chief evidence in favour of his views is as follows:—

1. The absence from the glacial beds of Essex of any *débris* representing a considerable denudation of the Weald during the glacial period, and the probability that the Wealden area was beneath the sea during the deposition of the Boulder Clay.
2. The comparative absence of Lower Cretaceous or Hastings-sand materials from the Postglacial gravel-sheets outside the north of the Weald.
3. The impossibility of reconciling the presence of Tertiary pebbles in certain Weald-gravels with an origin by means of streams flowing in the direction of the present rivers.
4. The antagonism between the character of the major valley of the Weald and that of any excavation producible by the agency of rivers.
5. The persistence of the old coast contour with the river-drainage entering it from the north.
6. The existence of a cause, in the shape of an isthmus at Dover, sufficient to induce a strong tidal scour.

November 23, 1870.—Joseph Prestwich, Esq., F.R.S., President,
in the Chair.

1. "On some points of South-African Geology."—Part I. By G. W. Stow, Esq.

In this paper, which was illustrated by numerous sketches, sections, tables, and specimens, observations were made on the stratification of the Jurassic beds of Sunday's and Zwartkop's rivers, resulting from researches made by Mr. Stow, with the view of determining the exact position of the several species of fossils found at the exposures on the cliffs of these rivers, and from this the sequence of the various beds. He indicated the existence of at least nine separate fossiliferous bands, pointing out the relative positions of the several *Trigonia*-beds, Hamite-beds, Ammonite-beds, &c.

He next treated of the so-called Saliferous beds of the district, and gives his reasons for regarding them as later in age than the *Trigonia*-sandstones above alluded to, and therefore not equivalent to that part of the series named "Wood-beds" by Dr. Atherstone.

Other researches of the author related to the Tertiary beds both inland and on the coast. He distinguished three zones on the coast later in date than the high-level shell limestones (Pliocene?) of the Grass Ridge and other parts of the interior. One of the coast-zones he named the *Akera*-bed, from the prevalence of a delicate species of that genus. Another zone was described as following the river-valleys in the form of raised terraces, characterized by the presence of a large *Panopæa*. The latest shell-banks have been thought to be Kitchen-middens; but the author regarded them as shore-deposits in place. The author concluded by tracing the probable climatal and geographical changes in this region during geological times, and indicated, as far as his material allows, the probable migrations of the Mollusca, especially of the *Venericardia* characterizing the Pliocene Limestone.

2. "Note on some Reptilian Fossils from Gozo." By J. W. Hulke, Esq., F.R.S., F.G.S.

The author described the remains of two reptiles said to have been brought from Gozo by the late Captain Strickland. One of them was a fragment of the symphyseal part of the slender mandible of an *Ichthyosaurus*, having teeth of precisely the same character as those of the form from the Kimmeridge Clay described by the author under the name of *Enthekiodon*. For this species the name of *Ichthyosaurus gaudensis* was proposed. The other was the skull of a species of Crocodile, for which the author proposed the name *C. gaudensis*.

3. "On the discovery of a 'Bone-bed' in the lowest of the 'Linton Grey Beds,' North Devon." By F. Royston Fairbank, M.D.

In this paper the author called attention to the occurrence of a thin bed of rock to the west of the harbour of Lynmouth, containing an immense number of fragments of bone, some of them of large

size, and associated with massive bodies which he regards as coprolites. The author proposed to call this the "Lynton Bone-bed;" and he thought that its discovery might throw some light on the relative age of the whole series of rocks of North Devon.

XLIII. Intelligence and Miscellaneous Articles.

ON THE CURVE OF A RIVER'S BED. BY SAMUEL SHARPE, ESQ.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

MY friend Mr. Alfred Tylor, F.G.S., having by careful measurement found that the beds of many rivers approach more or less to the parabola, I was led to inquire whether under any, and what, assumed conditions that curve would be the line formed by water falling from a hard rock and flowing over a bed of alluvial soil.

In this case V , the velocity of the water on the bed of the river, and R , the resistance of a grain of sand, vary at every different part of the curve. But the conditions of the proposition require that the difficulty with which a grain of sand is moved should be everywhere the same when the river's bed has been cut into the form which it afterwards keeps; that is,

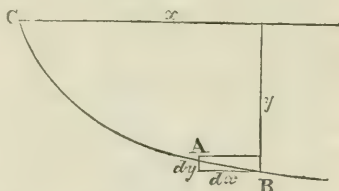
$$\frac{V}{R} = a, \text{ a fixed quantity.}$$

Let C be the point from which the curve starts, being the harder soil, which the water does not wear away, and $A B$ a portion of the curve $= V$.

Let y be the ordinate,

x be the abscissa.

Then $A B$ is the diagonal of a parallelogram of which dx and dy are the two sides.



The ease with which a grain is moved forward will be as $\frac{dy}{dx}$; and its resistance $R = b \frac{dx}{dy}$, in which b is a fixed quantity. Then

$$\frac{V}{R} = a \text{ becomes } \frac{V dy}{b dx} = a, \text{ and } V = ab \frac{dx}{dy}.$$

Then let the velocity of water falling down a perpendicular at B be taken as $= cy^2$, in which c is a fixed quantity; V its lessened velocity along the surface of the curve becomes $= cy^2 \frac{y}{x}$, supposing the water to reach B with the same velocity as if it had travelled along an inclined plane from C to B .

Then by uniting the two values of V , we have

$$ab \frac{dx}{dy} = c \frac{y^3}{x}, \text{ and } \frac{ab}{c} x dx = y^3 dy.$$

Then by integrating, $\frac{ab}{2c} x^2 = \frac{y^4}{4}$; and extracting the root,

$$2 \left(\frac{ab}{2c} \right)^{\frac{1}{2}} x = y^2,$$

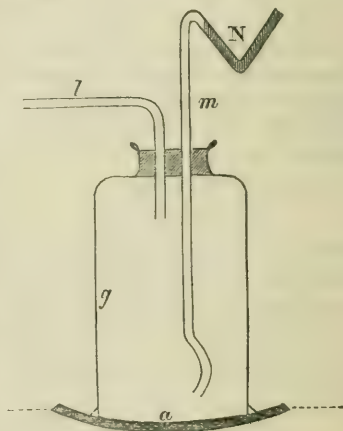
the well-known equation of a parabola.—Q. E. I.

SAMUEL SHARPE.

32 Highbury Place,
March 20, 1871.

A NEW EXPERIMENT, AND SOME REMARKS ON THE THEORY OF
LEIDENFROST'S PHENOMENON. BY E. BUDDE.

"Leidenfrost's drop is suspended when the vapour below it can support the pressure of the atmosphere plus the weight of the drop. If the former be removed, a far smaller tension must be sufficient to produce the phenomenon." This conclusion incited me to make the experiment of producing, by means of the air-pump, a *Leidenfrost's drop with water upon a support at less than 100° C.* With this view the following apparatus was constructed. Upon a copper dish (a) a glass bell-jar (g) about 8 centims. in diameter was cemented. Through a cork at the top of the bell-jar passed two glass tubes (l and m); the end of one of these (l), bent at right angles, just projected below the cork, while the other end was connected with the air-pump by means of an india-rubber tube. One end of the second tube (m) reached inside the bell-jar to near the dish (a); the other end, outside the cork, was bent somewhat in the shape of the letter N and was sealed. This bent part, which may be called N, was filled with water, of which it held about one third of a gramme.



The dish was placed in a water-bath, which kept it at a temperature of 80° to 100°. Suppose now the apparatus evacuated. By adequate exhaustion the water in N will liberate bubbles of air and vapour, which will partially collect in the upper closed part, and cause the water to flow down through the other end of the tube. This will fall either quite *boiling*, or very nearly so, on the dish, which is

heated considerably above its temporary boiling-point; the conditions are therefore favourable to the occurrence of a Leidenfrost's drop.

The experiment was made, and succeeded with the greatest ease. When the evacuation extended to a pressure of less than 10 centims., and the water-bath was raised to 90° C. or more, spheroids were obtained which moved briskly about. I was not concerned to ascertain what was the magnitude of the greatest spheroids which could thus be obtained; approximately the lowest temperature at which the drops maintained themselves was 83° C., while the manometer oscillated about 2 or 3 centims. Of course the tension increases when the evaporation commences upon the hot dish, and must again be diminished by pumping, which can scarcely be effected without the mercury oscillating. Sudden and violent formation of vapour, which might have endangered the manometer, has not occurred in my experiments, of which, to be sure, there were only three, and the quantities of water but small.

In repeating the experiment, it will always be advisable to keep the dish so far horizontal that the drop in its movement cannot touch the glass of the bell-jar. The bell-jar ultimately becomes dim from deposition of moisture, but at the lower and therefore warmer part remains long enough transparent to enable several drops to be successively observed. I think this experiment is decisive for the explanation of Leidenfrost's phenomenon; for it shows that the force which supports the drop obeys the laws of the pressure of vapours.

I may be here permitted to bring before a wider scientific public some observations on the shapes of Leidenfrost's drop which in March 1869 I communicated to the *Niederrheinische Gesellschaft für Natur- und Heilkunde*. As regards the formation of the star-shapes I adhere to Berger's explanation, with the addition, however, that the formation of the star is a phenomenon of vibration, and one accompanied by a remarkable formation of nodes. The motion in question is alternate extensions and contractions, usually occasioned by the collection of vapour under the centre (which causes the drop to spread out), brought into regular course and maintained by causes which Berger has described. The simplest vibration is that in which the drop extends and contracts as a whole; it then represents alternately a more flattened and an elongated spheroid. This modification is most easily witnessed with very small quantities of liquid.

If the drop has a larger area it behaves like any other vibrating body, and divides into aliquot parts, forming nodes and loops. The simplest division is that into quarters. It therein extends first in one direction so that its section is almost an ellipse. In the following period the larger axis contracts, and the former smaller axis extends, so that the section forms an ellipse at right angles to the first. It therefore oscillates between two crossed ellipses; and when the motion is rapid enough it is observed as a cross. When the amplitude of the oscillation is sufficient, the contraction in the middle extends so far that the section is biscuit-shaped; with greater amplitude the drop breaks up each time into two parts, which sometimes again flow together with surprising regularity, then extending in a direction at

right angles to the first, and again being broken up. If it forms 2.3, 2.4, and so forth nodes, it will be seen that on each elongation it forms a star with 3 or 4 rays and 3, 4, &c. indentations; if, then, the time between two elongations is small enough, a star of 2.3, 2.4, &c. rays will be formed.

The force which opposes the disruption of the drop is, beside molecular attraction, the pressure of the vapour which escapes at the side of the drop. Vapour will keep the drop so much the more together the more it is compelled by the form of the dish to press against its sides, therefore the more the dish is curved. The formation of nodes will be the more abundant the greater the resistance offered to expansion, and therefore the greater the curvature. It will thus be seen that the shape of the drop mainly depends upon the shape of the dish; and the following connexion is observed.

1. Very flat, almost plane dishes furnish with about 1 cub. centim. water ($\frac{2}{3}$ cub. centim. alcohol, $\frac{1}{2}$ ether) almost always the shape of crossed ellipses. If the curvature is very uniform, the shape remains constant as the drop diminishes. With large drops of water the motion is so slow that all the individual phases can be easily followed, especially the first rise of oscillations from a bubble of steam seeking to burst through. Alcohol and ether move more quickly.

2. More highly curved dishes give shapes with more than four nodes, the number of nodes increasing with the decrease of the radius of curvature, until at about $1\frac{1}{2}''$, owing to the diminution of the contents of the dish, there is again a decrease.

With the gradual diminution of the drop, which is caused by its evaporation, the circumference ultimately becomes too small for the original number of nodes; a pause then occurs, followed by new vibrations with fewer nodes. The above statements refer primarily to the largest forms, and in my experiments have been found to hold for these with such certainty that, by choosing the dishes, I could produce definite stars (to as many as 16-rayed ones) with a probability of more than $\frac{9}{10}$. Dishes not too smooth (copper for instance) are advantageous, since small roughnesses promote the vibrations. Very steady drops are obtained with small quantities of water (diameter of the spheroid 0.8 to 1 centim.) in a very smooth silver dish heated as gently as possible.

The formation of the smaller waves which almost always ruffle the surface of the drop, I should ascribe to the vapour, which in escaping brushes over the sides and bottom of a drop as a violin-bow across a string. These vibrations may also become audible; their tone is a deep, gentle humming, but definite; to hear it, all noise from the flame must be avoided, even the heating-flame, of course only after the dish has been so far heated that its temperature is maintained for a time; a confusion of the note with the crackling which takes place just before the drop bursts is not very possible.—Poggendorff's *Annalen*, No. 1, 1871.

THE
LONDON, EDINBURGH, AND DUBLIN
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

[FOURTH SERIES.]

MAY 1871.

XLIV. *On Barometric Differences and Fluctuations.* By JOHN KNOX LAUGHTON, M.A., F.R.A.S., F.R.G.S., *Mathematical and Naval Instructor at the Royal Naval College, Portsmouth*.*

PART I. *The Influence of Temperature and Humidity on Barometric Pressure.*

TILL within the last few years meteorologists invariably commenced any inquiry into the movements of the air by the statement, however differently worded, that all movement is caused by the unequal distribution of heat at the surface of the earth. This unequal distribution of heat was supposed to act by the unequal expansion and rarefaction of contiguous masses of air, by the consequent difference of their density, and the disturbance of equilibrium. From a hydrostatic point of view such a statement seemed theoretically perfect; and it was therefore laid down with the most absolute confidence as a meteorological axiom. Later and more exact observations, extending over a wider geographical field, have established its incorrectness, and have shown very conclusively that the mere expansion of air by heat, and its increase of density by comparative cold, do not account for the anemological phenomena which really take place, and in many instances seem to be in direct opposition to them. No one can doubt that heat, acting on the air, does produce very important results; but it is now equally impossible to doubt that, whatever its effects may be, they are so mixed up with the effects produced by other agencies (such, for instance, as evaporation or condensation), that no class of phenomena can be attri-

* Communicated by the Author.

buted solely to this property of expansion and contraction. That, according to this theory, wind blows, necessarily and invariably, from the place of less towards the place of greater heat, is contrary both to our geographical knowledge and to our common experience; and whilst loth to admit in plain language that the effect due to heat, in this manner, has been overstated, the leading meteorologists of the present day have virtually substituted for the old axiom a modification of it which may be shortly stated thus:—

The movements of the air are caused by differences of barometric pressure: air necessarily moves from the place of high pressure to the place of low.

And on this axiom, the system of all modern writers, more especially of Mr. Buchan of Edinburgh, and Professor Mohn of Christiania, is explicitly founded.

Before, however, we can unreservedly accept such an axiom, we must have a clear idea of its exact meaning; before we can admit that differences of barometric pressure are the causes of the atmospheric movements, we must have some insight into the cause of these barometric differences themselves; and it is this point which meteorologists, without exception, have slurred over: however lucid their writings may be in other respects, here their clearness fails them; and frequently the only inference that can be drawn is, that barometric differences are a first cause, beyond which it is not necessary to inquire. This, of course, is not what they mean; but, admitting that it is a question of great difficulty, they have rather avoided than grappled with it. With the various explanations, or partial explanations, that have been offered, no one can feel satisfied; and yet, till some satisfactory explanation is arrived at, meteorological research tends to become merely a dry repetition of the observations of phenomena which we can dimly see are connected in some way with each other, without being able in the least to trace that connexion to its source. Until the phenomena of barometric fluctuations and differences are understood, we are not at liberty to assume that they are the causes of atmospheric movements. We know that a barometric difference between two adjacent masses of air will, under certain conditions, establish a movement between the two: experimentally, we know that air will rush, even with great force, out of a vessel in which it has been compressed; but we also know that under certain conditions a movement of air may raise or depress the barometer—a fact that we may readily illustrate by driving a current of air, by a force-pump, into a glass vessel which contains an aneroid.

When, of two observed phenomena, either one can produce the other, very great caution is necessary before we decide which is

the cause, which is the effect ; and when, as in this instance, two classes of phenomena (atmospheric movements and variations in barometric pressure) are so closely connected, and stand so manifestly towards each other in the nearest relationship, in the absence of further information we have no means of deciding whether the changes in barometric pressure cause the atmospheric movements, whether the atmospheric movements cause the changes in the barometric pressure, or whether some third and extraneous agency does not cause both the movements of the atmosphere and the fluctuations in its pressure. These conclusions seem, at first sight, equally to merit our acceptance ; and notwithstanding the prescriptive right to our belief claimed for the first mentioned (by reason of old traditions and scientific conservatism), an examination into the grounds of this belief is at least necessary before we can yield an entire and rational assent.

It is therefore the causes of barometric fluctuations that I propose to inquire into ; and since in meteorology, more almost than in any other science, incorrect statements have been frequently put forward, resting on some plausible theory, the fallacy of which it is difficult to point out, I propose submitting each step in our inquiry to the test of experimental and geographical fact : if it cannot stand this, it is good for nothing ; if it can, we may consider it a certain, however slight advance towards establishing our conclusions on a satisfactory basis.

At the very outset it is necessary to call attention to the fact that the barometer does not measure weight : the name unfortunately implies that it does ; but we all know (if we can only bear it in mind) that it does not. What the barometer measures is elastic force ; with weight or density it is concerned only so far as they are functions of elastic force. The distinction between weight and elastic force cannot be too strongly insisted on ; and the want of observing it has led to great confusion. It has thus been almost universally assumed that heat lowers the barometric pressure, and that cold increases it : I find this repeatedly stated in the most absolute manner by meteorologists of high repute ; and yet, as an abstract fact, it is distinctly contrary to the truth. Heat, while it tends to increase the volume, does certainly tend to decrease the density ; but while exercising this tendency, it actually increases the elastic force of the air. Cold (understanding by cold merely a diminution of heat) works in exactly the opposite way ; it tends to decrease the volume and to increase the density, but does meanwhile lower the elastic force. If, then, the increase or decrease of volume does not keep pace with the increase or decrease of temperature, the barometric pressure is raised by heat and lowered by cold. This is easily

enough shown by putting an aneroid barometer, with thermometer attached, under a glass receiver: the barometer, which at first will show the pressure of the air in the room, will rise if hot water, fall if iced water is poured freely over the receiver; the difference of pressure corresponding to a difference of temperature can be exactly calculated by the well-known formula; but independent of any such careful experiments or calculations, it is within the familiar knowledge of all of us that if a blown bladder be held to the fire it will burst, if it is taken out into the cold it will shrink and become limp and flabby; the elastic force of the air inside is increased or diminished by the change of temperature.

From a meteorological point of view, the question then presents itself, whether, in the free atmosphere, expansion takes place in such a way as to cause the increase of volume to exceed the proportionate increase of elasticity, so as, in fact, to lower the elastic force. The view apparently taken by meteorologists is that it does; it is only by such a view that the statement laid down by Mohn, Buchan, and others can be supported*; but considering the subject as, in the first place, a branch of practical geography and not of abstract theory, I find no satisfactory basis for such an opinion; or rather, I should say, I find many instances which stand diametrically opposed to it.

Foremost amongst these, I may name the Hot Wind of New South Wales and of Victoria. This wind, which bursts out at intervals from the deserts and burning plains of Central Australia, itself dry and burning almost beyond conception, can be attributed only to a great and sudden increase of elastic force. It would be foreign to my present subject to speak of the peculiar properties of this wind; its sudden outburst as a hot wind is all that I have now to do with; and this sudden outburst seems to me incapable of any other explanation than as the effect of a remarkable barometric rise in the interior of the continent. The mass of air, heated to an extreme degree, and having such aqueous vapour as it contains similarly superheated, has its elastic force enormously increased; it tends to expand with violence, and bursts out towards the place of least resistance; and it seems to bear strongly on the question I have proposed, that it finds that place of least resistance always, or almost always towards the south—that is, towards the place of greatest relative cold. There is not, I believe, any record of a Hot Wind having been experienced on the north coast of Australia; and Stuart, in describing the stony desert which, in his opinion, is the birthplace of the Hot Wind of the south, says very explicitly that, after passing this place, the Hot Wind, which before had been frequent, was never felt till he again

* *Atlas des Tempêtes*: Christiania, 1870, p. 19. Handy Book of Meteorology, second edition p. 47.

passed the place on his return journey*. Whenever the Hot Wind breaks out over South-eastern Australia, it is invariably and immediately followed by a strong cold gale from the south. It is impossible to avoid comparing this with what takes place on opening an oven door: there is, first, a strong outward rush of heated air, then an inward rush of cold air—an alternation which is repeated two or three times with continually diminishing force before equilibrium is established. The same alternation of air-currents was shown very exactly by the late Professor Daniell†. He connected two receivers by a glass tube in which was a stop-cock. From one (B) he partially exhausted the air; into the other (A) he compressed an additional quantity,—thus making a very marked difference in the elastic force of the two volumes. On opening the stopcock, the air rushed violently from A to B; this rush was immediately followed by a return from B to A, the alternation in the glass tube being distinctly visible. It is evident, therefore, that air putting itself in motion by the exercise of its elastic force, generates in itself such a momentum that far more will yield to the impetus and follow in the same direction than is called for to restore a balance of pressures: the elastic force becomes least in the chamber where it was greatest, so that there is a return movement, and even, when the first difference has been excessive, several alternations of the movement. I see no reason to doubt that the Hot Wind of Australia and the cold southerly gale immediately following it are natural instances of the same thing on a very large scale—that the Hot Wind, bursting out from the desert by reason of an elastic force immensely increased by an extraordinary accession of heat, drags in its volume so much air that, as the impetus dies away, it leaves behind it a comparative vacuum, into which the cold air from the south rushes with great violence. We have absolutely no barometric observations from the interior of Australia; even if we had, they would be of little value, from our ignorance of the level; but it seems in the highest degree probable that, when exact observations can be made, we shall find that a noticeable rise precedes the Hot Wind, and that a fall as marked immediately follows it.

I conceive that the Scirocco of the southern parts of the Mediterranean is, in its origin, exactly similar. It is, as the other, a hot wind off a burning desert, and is, like it, invariably followed by a cool wind from the opposite direction. Still, here again we have no barometric observations; so that I am unable to speak of the rise or fall of the barometer as ascertained facts.

On the other hand, however, the fact seems sufficiently established that in high southern latitudes the barometer stands, on

* Journals &c. pp. xiii, 473.

† Meteorology, vol. i. p. 49.

the average, nearly an inch lower than in other parts of the world. But over an area so vast as the Antarctic, and so cold even in the summer months, the air must be contracted to an extreme density; and though the temperature is probably much greater than that of Siberia or the eastern steppes of Asia during the winter, it is still very far below that of any other portion of the southern hemisphere; so that if increase of density necessarily led to a corresponding increase of elastic force, the observed barometric pressure in the Antarctic would be markedly higher than in any other part of the southern hemisphere, not lower.

The more exact our observations become, the greater and more persistent is the difficulty which opposes the idea that cold increases barometric pressure. Mr. Buchan, in his isobaric charts, shows a large patch of high pressure during the winter in Eastern Siberia, and at the same season a patch of low pressure in the neighbourhood of Iceland. These differences he considers due to the extreme cold of Siberia and the relative warmth of Iceland*. But by what principle of selection, according to such a view, does the barometer choose Iceland as its centre of minimum pressure? for the south-west corner of Ireland, at no very great distance, has a very much higher average winter temperature than the western side of Iceland; and still further to the south-west, towards the Azores, the temperature is still higher; so that even if, leaving other considerations momentarily on one side, we were disposed partially to admit that the extreme long-continued and unbroken cold of Siberia does permit an accumulation of air there, and produce this remarkable increase of barometric pressure through the winter, we cannot admit that the relative warmth of Iceland (relative only to Siberia) can so reduce the barometric pressure in that neighbourhood as to render it feeble relatively to that of much warmer districts neither remote nor separated by geographical barriers.

Many other instances might be brought forward; but those I have mentioned afford sufficient proof that heat, even long continued over a wide area, does not necessarily establish a relatively low barometer; nor does long-continued cold establish a high. This is in conformity with our everyday experience on a small scale; for we know that the barometer is by no means necessarily higher in cold weather than in hot, and that its mean height in England is very much the same in winter as in summer. There is, as I have shown, reason to believe that a rise of temperature does in some instances affect the barometer and cause an alternation of high and low pressure; but there seems no support in geographical fact for the opinion that continued high or low temperature sensibly affects the barometric mean of the district subjected to it.

* Handy Book of Meteorology, p. 40.

Another error due to the confusion between the ideas of density and elastic force is the opinion, widely put forward, that the presence of aqueous vapour in air reduces the barometric pressure, and that this becomes lower accordingly as the quantity of vapour becomes greater. It is said that aqueous vapour is lighter than air, and that, since a volume of air saturated with vapour has less weight than an equal volume of relatively dry air subject to the same pressure, the barometric pressure of the moist air is therefore necessarily less than that of the dry—as if between volumes of air balancing the same pressure there could be any difference at all! In fact the very important clause, *subject to the same pressure*, is overlooked; so that it is carelessly but very commonly maintained that the mere presence of vapour in the air accounts for a low barometer, or even that the introduction of vapour into the air causes a fall. How utterly opposed this last idea is to fact can at once be shown by putting an aneroid under a glass receiver containing comparatively dry air, and introducing water: the vapour thrown off raises the barometric reading by the amount due to the vapour at the existing temperature, gradually increasing it till the air inside the receiver becomes saturated. If in a wider field we refer the question to geography, and inquire whether those regions of the earth where the air is permanently saturated with moisture are the regions where the barometer is also permanently low, we obtain answers of the most widely differing purport. Great stress is usually laid on the low barometer in the Doldrums of the Atlantic, or in India during the season of the south-west monsoon; but, on the other hand, in the valley of the Amazon and some of its principal tributaries, where, according to Mr. Bates, “one lives as in a permanent vapour-bath,” we find travellers repeatedly noticing that the barometric observations give a level scarcely above that of Para*. Comparing these opposite instances with those already mentioned, of air almost entirely free from moisture showing a very high pressure, as in Siberia, or a very low pressure, as in the Antarctic, we can scarcely form any other conclusion than that neither the permanent presence of moisture in the air nor its permanent absence produces any effect on the barometric mean. This conclusion corresponds with that at which we have previously arrived with regard to long-continued high or low temperature; and though both rest entirely on geographical observation, it will be not uninteresting to examine into the cause of results so contrary to the general, or rather to the popular belief.

In the reasoning which has been adopted, there is clearly a

* Amongst many others, see Wallace, ‘Travels on the Amazon and Rio Negro,’ p. 411; Chandless, in Journ. of the Roy. Geog. Soc. vol. xxxvi. p. 94.

fallacy which has escaped detection, until a rigorous application of the test I have proposed (that of geographical comparison) compels us to recognize its existence. It appears to me to lie in the unauthorized assumption that any increase or decrease of temperature or humidity which affects the lower strata of the atmosphere extends upwards to its very furthest limits. This assumption may probably, to a great extent, be attributed to the incorrect way of speaking now so general, and which indirectly teaches us to think that a mass of air being made specifically lighter than it was, whether by expansion from heat or by expansion consequent on the intermixture of aqueous vapour, has a tendency to rise, and does rise. We know that this is not the case: we know that a piece of cork at the bottom of a basin has no tendency to rise, and does not rise; but if water is in the basin, the cork is raised by the pressure of the water underneath it. But popular usage says that the cork rises; and it would now almost savour of pedantry to insist on the more correct expression. Still we may and must insist on a clear understanding of the sense in which the word is used: custom may modify the laws of grammar; but the laws of gravitation are beyond its power.

It is thus, however, that a mass of air heated or rendered more humid has been supposed to rise, as though by virtue of some quality imparted to it which causes the earth to repel it; yielding to this, it rises and continues to rise to the very top of the atmosphere, where its power of rising suddenly ceases; the repellant force is changed into one of attraction; and the mass of heated air, finding itself above the air immediately near it, flows over it, and increases the height and therefore the weight of the adjoining atmospheric column. Put into plain and unmistakable language, this statement would shock the common sense of any one with the smallest pretension to scientific knowledge: it is none the less the statement which is repeated in almost every work on popular meteorology, as well as in many of a much higher order of merit. We know that it is not true; for we know that a mass of air, whether hot or cold, moist or dry, has absolutely no tendency to rise, but, on the contrary, is drawn down by all the force of gravity. We know that it is not true; for countless observations in almost every part of the earth prove to us that the height to which the heat or humidity of the air extends is very limited. An explanation of the way in which strata of hot and cold, of moist and dry air are intermixed is one of the most curious and interesting problems in meteorology; but that they are intermixed, that a hot stratum, or a wet stratum, or any other stratum is found frequently lying between two strata of a very opposite condition, forming a sort of atmospheric

sandwich, is perfectly certain. The balloon observations of Mr. Glaisher have, at any rate, established this beyond the possibility of further doubt*; but they have also established the fact that above all, beyond the reach of these alternations, is a region where the air is intensely cold and intensely dry. I have elsewhere† discussed at great length the peculiar unwillingness with which two masses of air, or any fluid, mix when their thermometric, hygrometric, or other conditions are different. The distinct outline very commonly offered by a stench or a perfume affords a clear and familiar illustration of my meaning: we do not, as a rule, find the smell gradually grow on us; it comes with such a sharpness that its rate of progress could easily be timed. It is the same with masses of air lifted into the upper regions of the atmosphere. A mass of hot air or a mass of moist air retains as much of its heat or moisture as is not by the forcible expansion withdrawn from it; and refuses, by virtue of this property, to mix with the cold or dry air which lies above it, or which it drags up after it.

It must therefore be borne in mind that when a certain mass of air is heated, and a tendency to expand—not to rise—imparted to it, its elastic force, which is the measure of the tendency to expand, is increased, and exerts itself in that direction in which at the moment it finds least opposition. Any one who has watched the course of a stream will know how the most trifling and apparently the most insignificant obstacles deflect its flow in a very marked manner; a blade of grass lying over from the bank and just tickling the surface will make a ripple and an eddy and a backwater, producing an amount of disturbance which repeated observation can alone lead us to credit. A tiny bit of gravel thrown into a pool will send waves swelling and circling almost as far as we can see; and knowing that these effects are produced in a comparatively dense sluggish fluid such as water, we may, without extravagance, maintain that causes infinitesimally small, and altogether beyond our power of discernment, will suffice to direct the motion of a fluid so mobile and so elastic as air. It therefore by no means follows that the expansion of heated air takes place in an upward direction. We have, as I have shown, strong reasons for believing that it sometimes takes place laterally; but these cases are exceptional, and may fairly be supposed due to a very extraordinary and sudden increase of temperature; but when an increase takes place slowly and steadily, the elastic force and the expansion would appear to keep pace with each other; the mass of air immediately over-

* See also Strzelecki, 'Physical Description of New South Wales,' p.162.

† Physical Geography in its relation to the prevailing Winds and Currents, pp. 149 *et seq.*, 266.

head is to some extent lifted; the air all around, or principally in one direction, is to some extent driven back; and equilibrium of elastic force being maintained, equilibrium of density is also maintained by a slow and for the most part imperceptible change between the hotter and colder masses; for geographically, and more especially at sea, the large areas of hot and cold air are not separated from each other by any sharp outline; the differences of temperature, and therefore the differences of density, are very gradual, and any motion that differences of density may cause must be of an extremely gentle nature.

This statement is very contrary to the common idea and the generally received theory; but it is easy to show that any velocity which can possibly be generated by such differences of density is altogether inappreciable. The most familiar illustration of this may be taken from the trade-winds. Premising that the account given of these winds in every book on meteorology with which I am acquainted is more or less incorrect in its geographical details, I will, so far as the data on which my calculation is based, follow Mr. Buchan, the clearest and latest exponent of the influence of density. He says that in the Atlantic the north trades prevail between latitudes 9° and 30° *. According to his charts, the mean pressures on these parallels may be taken as 29.95 and 30.15, and the mean temperatures as about 80° and 72° F. He gives no hygrometrical estimate; but wet-bulb temperatures of 79° and 68° will probably not be very far wrong. These elements give us, from Mr. Glaisher's Tables, the weights of a cubic foot of air on each parallel as 509.5 and 522.7 grains respectively, showing a difference between the two of 13.2 grains. Bearing in mind that a cubic foot of air on the parallel of 30° presses on the cubic foot of air immediately south of it, and not on a cubic foot on the parallel of 9° , it follows that the difference of weight between two contiguous cubic feet is $\frac{1}{6000}$ of $\frac{1}{60}$ of $\frac{1}{21}$ of 13.2 grains, or $\frac{1}{600,000}$ of a grain nearly. The acceleration due to this pressure is $\frac{1}{600,000 \times 500} \cdot g$ nearly, or rather less than $\frac{1}{900,000}$ of a foot per second; which, taking no account of friction, or the absorption of motion by compression, elasticity, or in any other way, would be equivalent to about 8 inches in an hour. It can scarcely be a matter of opinion whether a force so small is in reality capable of overcoming the numerous natural hindrances to the motion it would tend to generate.

The fact is that we bring to the study of meteorology ideas as to the motion of air which we have formed from noticing the draughts in a room, or the strong currents generated in the process of ventilating coal-mines; we forget the effect not only of

* Handy Book, p. 214.

the much greater difference of temperature, but of the immediate contact of the two bodies of air. In a coal-mine, for instance, between the air that, passing through or over an enormous fire, is driven up the upcast shaft and the air that drives it up, there is a difference in temperature of perhaps not less than 200° F. The difference in the weight of a cubic foot of air corresponding to such a difference of temperature is about 184 grains; and the weight of a cubic foot of the hotter air would be something less than 400 grains. Thus we have the acceleration produced as about 15 feet per second. Such a calculation has, of course, no pretensions to accuracy; my object here is, not to arrive at the velocity with which air issues from the mouth of a coal-pit, but to show that, between the pressure which causes the current of air in a coal-pit and the pressure which has been supposed to cause the trade-winds, there is practically no comparison, and that the latter force is utterly inadequate to produce any appreciable effect.

I have been obliged to dwell on this point in order to establish the fact that there can be no perceptible barometric difference due to the lifting effect of the interchange resulting from differences of density. A low barometer caused by a high temperature is therefore really caused by a further expansion consequent on the withdrawal of a pressure from above, by the overflowing of such air as, by the expansion of the lower strata, is lifted above the strata of equal pressure. A method of calculation similar to that already adopted enables us to form some idea of the numerical value of this overflow. A cubic foot of moist air at 72° F. is increased at 80° F. to 1.008 foot, which gives a linear expansion of from 1 foot at 72° to 1.0027 at 80°. If, therefore, we suppose this expansion to extend through a height of 10,000 feet (a very exaggerated estimate), the superior strata on the parallel of 9° are raised 27 feet higher than the strata of equal pressure on the parallel of 30°. The incline down which the overflow pours is therefore one of 27 feet in $21 \times 60 \times 6000$, or 1 foot in 280,000. The motion generated by gravity on such a slope, with the very intense friction which masses of air exercise on each other against it, cannot possibly be perceptible; it can neither become a sensible wind, nor can it show itself by any difference of barometric pressure.

I have already called attention to the fact that geographical observations directly contradict the statements that a high temperature or a humid atmosphere cause a low barometer, and that, on the other hand, a low temperature or a dry atmosphere cause a high one. So far as geography is concerned, we may regard it as clearly established that there is no necessary relation between the mean reading of the barometer and the mean readings of the

thermometer or hygrometer : the calculations I have just made show with equal certainty that the forces called into action by the differences of expansion or of density due to mean thermometric or hygrometric conditions are quite inadequate to produce any sensible motion ; we are therefore fully warranted in the conclusion that permanent high temperature, permanent humidity, or the conditions most opposite to these, are *not* the causes of local differences of barometric pressure.

PART II. *The Geographical Distribution of Barometric Pressure.*

It is not, however, to be doubted that mean barometric pressures have a very marked geographical distribution ; and though concerning these our information is very incomplete, and, even where it might seem complete, is frequently incorrect, we are still able to determine some of the principal features of this distribution with tolerable accuracy. The best-known instances of it are perhaps the low barometer in the Doldrums of the Atlantic, in India during the summer months, in the Antarctic and in parts of the Arctic ; and, on the other hand, the high barometer in the upper valley of the Amazon and its tributaries, in a patch of the North Atlantic between the Azores and Bermuda, in a corresponding patch of the South Atlantic to the north and north-west of Tristan d'Acunha, and in Eastern Siberia during the winter. There are other districts, perhaps equally important ; but these I have named may be considered, to a certain extent, representative areas, offering, at any rate, very many of the most noticeable barometric phenomena.

A consideration of the climatic characteristics of the Doldrums or of India seems to point to the heavy rains of these localities as affording some explanation of their low barometer. Remembering the extent and absolute, as well as relative, amount of the precipitation which, during the wet season, takes place in India along the southern foot of the Himalayas, along the whole of the west coast, and further east in the province of Khasia, and thence southwards to the sea, it is difficult to overestimate the sudden disturbance to which the atmosphere is subjected, whether merely by the removal of the vapour and the consequent reduction of elastic force, or by the expansion following the enormous development of heat by condensation. The observations of this great rainfall are neither sufficiently numerous nor consecutive to admit of our assigning to it any exact measure ; but we are certainly far within the mark when we say that it does not average less than one inch in the day during the summer months. Now 1 cubic inch of water weighs 253 grs. ; so that an inch of rain falling on an area of 1 square inch im-

plies that 253 grs. of vapour are withdrawn from the column of air that rests on it, and almost entirely from that part of the column which extends upwards from the ground to a height of 10,000 feet. If we suppose the mean temperature throughout this column to be 55° F. (and a few degrees more or less make scarcely any difference), the 253 grs. of vapour have a volume of 89,770 cubic inches; that is to say, the vapour and the elastic force belonging to it have been withdrawn from the atmospheric column through a height equivalent to 89,770 inches, or in round numbers 7500 feet. This, from a column 10,000 feet high, denotes the withdrawal of three-fourths of the elastic force of the vapour, that is, of a pressure equal to $\frac{3}{4}$ of an inch of the barometric column.

On the other hand, the heat given off by the condensation of 1 pound of water is sufficient to raise 1000 pounds of water, or 4200 pounds of air, one degree in temperature. The pound of water, as rain 1 inch deep, covers an area of about 28 square inches; $\frac{1}{28}$ of this heat is therefore applied to the column on the base of 1 inch which we have been considering, or sufficient to raise the temperature of 150 pounds of air 1° F. Now the whole column, to its highest limit, weighs 15 pounds; so that the liberated heat, if applied to the whole column, is sufficient to raise its temperature 10° F. But there is no reason to suppose that it is so applied. We know familiarly that rain cools the air near the surface, which comes within the reach of our daily experience; and we know that frequently, when rain is falling on low-lying land, we find snow falling on land a little higher; so that we are justified in saying that the heat of condensation does not strike downwards towards the surface of the earth. It would therefore appear probable that the greater part of this heat is absorbed by the air in the immediate neighbourhood in which the condensation takes place; and supposing that it acts principally on that part of the column which lies between 5000 and 10,000 feet from the ground, it acts on a weight of air which may be considered as one-sixth of the whole, or as 2.5 pounds, and is sufficient to raise its temperature 60° F. It would be useless to carry a calculation such as this any further. The results obtained from data necessarily extremely inexact, are of little numerical value; but they serve to show dimly the extraordinary amount of the force suddenly let loose high overhead. Heat so applied to a column of air 5000 feet high cannot be compared in its effects with that which causes a rise of temperature, even through the same number of degrees, near the surface of the earth, where it acts only on the very lowest strata, and is diffused upwards by a process comparatively slow—so slow, in fact, that the mean

temperature of the air diminishes as we ascend at the nearly uniform rate of 1° in 300 feet, whilst the noontide temperature diminishes much more rapidly. I have already shown that differences of temperature, such as we ordinarily find near the surface of the earth, shading into each other through almost infinitesimal gradations through very great distances, are insufficient to produce any appreciable effect; but I am none the less ready to admit that the sudden expansion, equivalent to a rise in temperature of 60° F., of a column of air 5000 feet high and covering a base of hundreds of thousands of square miles, partaking, as it must do, almost of the nature of explosion, is capable of producing an effect the vastness of which may be conceived without entering into any detailed and not very exact calculations.

Whether, therefore, we consider merely the withdrawal of the elastic force, or the disturbance caused by this expansion, or, as in fact we must do, both combined, it would seem conclusively established that tropical rain is a cause sufficient to produce a low atmospheric pressure in any given locality during the wet season: but we are met by the very startling contradiction offered by observations in the valley of the Amazon, where the volume of water that rolls down the river swelled to 50 feet above its level, whilst the whole country for many miles on each side of the principal tributaries is inundated, tells of the extraordinary precipitation, to which a total want of meteorological registers prevents our assigning any numerical estimate. Mr. Buchan, indeed, runs his mean isobar of 29.9 up the whole length of the valley, across the Andes, and down to the Pacific on the western side; but admitting this as the mean height of the barometer at Para, we cannot allow that it is also that of the interior, unless we reject the observations of Chandless and several other careful travellers. Wallace, in attempting to form an estimate of the height of various stations on the Rio Negro, says that "the mean of five observations at Barra, some with river-, some with rain-water, gave $212^{\circ}.5$ as the temperature of boiling water," which is equivalent to a barometric height of 30.2 inches. Chandless, again, says that on the Purus, "at Canotama, three feet above the high-water line, the barometer (mean of sixteen days at 9 A.M. and 3 P.M.) stood higher than in Manaos at about 10 feet above the high-level of the River Negro at the homologous hours."

Mr. Chandless's observations were taken in July, Mr. Wallace's in August, and compel us, in the absence of others with superior claims to exactness, to believe that the barometer does stand very high at that season of the year. But that season is not the rainy season; the rains on the Purus end in the early part of June, when the whole country is under water, forming

an enormous lake or series of lakes, the river is many feet above its level, and boats cross freely to and from the Madeira. Castelnau's maps of this country are in this respect quite in accord with Chandless's description; they both show that the land is, for the time, virtually turned into an inland sea. Mr. Wallace's description of the country adjoining the Rio Negro is to the same effect; the upper parts of the Amazon and of the Orinoco exhibit the same peculiarity; the very remarkable flatness of the whole of this vast area retains the water, so that it runs off with extreme slowness, and meantime much of it is flying off by evaporation. The temperature is uniformly high, and the quantity of aqueous vapour the air can contain is very great. Its elastic force, indeed, tends to disperse it; but aqueous vapour does not easily diffuse itself through air that has already a high degree of humidity; it spreads itself into the superior strata slowly and apparently with difficulty, even when the air above is far removed from the point of saturation*. And here the only direction in which it can spread is upwards: the Andes, to the west, are impenetrable; on the north the mountains of Venezuela are nearly so; the ridge of elevated land on the south offers an obstacle that checks, though it does not prevent, the passage of air; and the trade-wind blowing fresh up the river, not only drives it back on the east, but brings in new and comparatively dry air†, the elastic force of which the excessive evaporation rapidly increases. It would seem therefore probable (and until we have more numerous and extended observations we must rest on probability) that the barometer is high only during this season of evaporation, that it is even low during the rainy season, and that during the rest of the year, in the interval between the draining off of the water and the return of the rains, the barometer has a standard height concerning which, at present, we have no reliable information; but now that steamers run up the Amazon almost to the foot of the mountains, we may hope that observations of the barometer and wet- and dry-bulb thermometers will enable us before very long to speak with more confidence‡.

It seems, however, as I have said, tolerably certain that the barometric pressure in the upper valley of the Amazon is very

* See a Note by the Astronomer Royal in *Proc. of Brit. Met. Soc.* vol. i. p. 365.

† At this season, too, the equatorial current over which the trade-wind has passed is at its minimum of temperature, so that it supplies even less vapour than usual.

‡ The whole question, in its relation to the level of different stations on the Amazon, is discussed at great length by Professor Orton in a paper on "The Andes and Amazon" in *Silliman's American Journal of Science*, 2nd series, vol. xlvii. p. 203.

high in the months of July and August; and it is quite certain that the wind at that season blows strong up the river*—blows towards the place of high pressure, not from it. One direct and unmistakable contradiction is sufficient to overthrow any axiom; and an instance such as this deals a most severe blow on the particular axiom which has been proposed for the guidance of meteorologists. If wind does not always blow from the place of high pressure, but in some instances blows straight on end to it, it is quite clear that the one part of the proposed axiom is false, and that we are at liberty to reject the other—to say, in short, that winds are not always caused by differences of barometric pressure. Winds cannot blow from the place of low to the place of high pressure if differences of pressure are the cause of the winds; when, therefore, we find winds distinctly and for a length of time blowing towards the place of high pressure, we feel sure that they are not doing so by reason of any mere disturbance of hydrostatic equilibrium; we are confident that the difference of pressure is not the cause of such winds, although, indeed, such winds may well be the causes of the difference of pressure.

The valley of the Amazon, however, is only one locality in which the meteorological axiom I have referred to is palpably controverted. Nor is it the most decided. A still clearer instance of the failure of the law is offered by the high barometer of Eastern Siberia in the winter months. One of the so-called poles of cold is in this district; and it has been conjectured, and indeed very positively stated, that the extreme cold, by increasing the density of the air, is the direct cause of the high pressure—an opinion which I have shown to be neither theoretically correct nor conformable to geographical evidence; for the barometer near the other northern pole of cold, in North America, stands low, as it does also in the antarctic regions. Leaving, then, the low temperature out of consideration, as a condition which cannot possibly produce such very opposite effects, and examining into the points of difference between these places which resemble each other in their extreme cold, we find that to leeward of Siberia is a lofty and difficult extent of mountainous country. The prevailing wind of winter is west or north-west; and almost impenetrable mountain-ranges bar its progress towards the east or south. Into the angle between these ranges air is continually driven, and acquires an elastic force tending towards equilibrium, not against the statical pressure of the air to the westward, but against the dynamical pressure of the air

* Bates, 'Naturalist on the Amazons,' vol. ii. pp. 13, 21. Maury, 'Barometric Anomalies about the Andes' in Silliman's *American Journal of Science*, 2nd ser. vol. xix. p. 385.

moving from the west. Much of the elastic force due to this compression is lost by some outward movement, whether upwards, or eastwards through the mountain-passes, or in occasional bursts to the westward, when for a time it drives back the advancing current; but the prevailing motion continues, or speedily resumes its course; and the barometer is kept at this very remarkable height throughout the season of strong westerly winds. Mr. Buchan has endeavoured to account for our easterly winds of spring by a reference to this high pressure*; but he has overlooked the difference of seasons. The easterly winds of this country are the marked feature of April and May; the high barometer of Siberia is almost peculiar to January. In January we do certainly sometimes get strong easterly winds bringing hard frost; but they are exceptional, and westerly gales (such as the 'London' foundered in in 1866) or soft westerly winds (such as put almost half England under water in 1869) are the more recognized type of January weather. In April and May the barometer in Siberia has fallen to the comparatively low mean of 29.96 inches; but it is then that the air, warmed and expanded by the advancing sun, and no longer forced back by the full strength of the winter west wind, breaks out over the whole of Europe, reaching even to us in the most distant corner of it. I will not at present stop to argue that the strong west wind is really the cause of this high pressure in January; but whether it is or is not, it is equally certain that at that season the winds, not the local, diurnal, and irregular variations, but the prevailing winds of Northern Europe and Asia blow towards the place of high pressure†, not from it.

During the summer months the case is almost entirely reversed. The west winds of Europe are, as compared with those in winter, both irregular and of very moderate force; but the barometer in Siberia has at that season a remarkably low mean. The feeble pressure, far from causing a rush of air towards it, permits the previously existing rush to die away; it is as unable to increase the force of the summer breezes as the high pressure is to control the fury of the winter gales. Relative high temperature is again said to be the cause of this area of low pressure; but, independently of the reasons already adduced, Siberia is not an area of relative high temperature. Arabia might be called so, or the Punjaub, or the Sahara; but certainly not Siberia, with, in July, a mean temperature of 70° F., a mean which runs nearly on a parallel of latitude across the whole of Europe and Asia; so that, looking at an isothermal map, and bearing in mind the

* Handy Book, p. 233.

† At Barnaul, for instance, Mr. Buchan tables 20 days of west wind in January and 3 of east.—Trans. of Roy. Soc. of Edin. vol. xxv. p. 622.

Phil. Mag. S. 4. Vol. 41. No. 274. May 1871. 2 A

continuous range of mountains on the south, there seems absolutely no reason, so far as temperature is concerned, why the area of low pressure should not be in Germany or Russia instead of in Siberia*. But in other respects there is a wide difference between the summer climate of Central Europe and Eastern Asia. The rainfall, which throughout Russia is trifling, and on the plains of Siberia is scanty, is very great on the mountain-slopes to the south and east. "In the neighbourhood of Riddersk in the Altai Mountains, the dew falls so heavily that the dress of the horseman is completely saturated when riding through the high grass, while in the sombre forests of the north-western Altai, called locally Taigi, the atmosphere is still more humid, and rain during some summers falls incessantly"†. Such rain acts here in the same way as the still heavier rain acts south of the Himalayas. Wherever aqueous vapour is largely withdrawn from the air, the elastic force must be reduced, even though the reduction may not always be so great as to affect the barometer; and in this instance, since we know that in July the aqueous vapour at Barnaul has a mean tension of $\cdot 43$ of an inch, that the air in Thibet is excessively dry, and that on the Thian Shan Mountains dew and rain are almost unknown even during summers which on the Altai are unusually wet‡, we must admit that the northerly and north-westerly wind which brings the vapour into Siberia is entirely deprived of it as it passes out.

But whilst the low barometer of Hindostan, of the Doldrums, or of Siberia may thus be explained by the free and excessive condensation of aqueous vapour, it is impossible in any similar manner to explain the low barometer of the Antarctic, a district which offers a decisive contradiction to every law which meteorologists have proposed: it is an area of extreme cold, yet the barometer is not high; it is an area of low pressure, but the prevailing winds do not blow towards it. What the actual height of the barometer or thermometer may be within the icy region we have no means of knowing; but it may fairly be supposed that they are both very low; and it is quite certain that the pressure diminishes steadily from the parallel of 50° southwards, until barriers of ice hitherto impassable put an end to our observations.

The enormous quantity of snow which overlies the ice and land attests the occasional presence and condensation of aqueous vapour in the air; but the temperature in the higher latitudes diminishes very gradually, and the tension of the vapour in such air as is swept from warmer seas into the icy regions must be so

* Buchan, 'Handy Book,' p. 40, plate 4. See also 'Nature,' vol. iii. p. 76.

† Journ. of the Roy. Geog. Soc. vol. xxxv. p. 227. ‡ Ibid. *in loc. cit.*

feeble that the withdrawal of even the whole of it could not produce any marked effect. But northerly winds are themselves rare, winds from the south, from the east, or from the west alternating irregularly south of the parallel of 65° ; and it must be remembered that the immense snow fields are the accumulations of years, perhaps of centuries, and that the actual increase each year is probably small.

It has often been suggested that the centrifugal tendency due to the rotation of the earth may possibly be the cause of this singular depression of the barometer; but the suggestion has met with but little favour, because, if the centrifugal tendency occasioned a low barometer in the neighbourhood of the south pole, it might fairly be expected to occasion it also near the north pole, and it has been very positively said that in this last-named region the barometer is not low. This statement is incorrect. In most places within the Arctic circle where we have observations, the barometer does stand very low; and although the depression does not seem to be either so great or so regular as in the Antarctic, it is none the less a very well-established fact. I have therefore no hesitation in agreeing in great measure with those who would refer it to a centrifugal tendency, believing that the irregularities which occur in both regions, but more especially in the Arctic, are in most instances capable of satisfactory explanation.

The attempt to calculate accurately the amount of depression at the poles which this centrifugal tendency should occasion seems at present quite hopeless; the results which have been obtained have, for want of satisfactory data, been so utterly erroneous that they have no value whatever. But leaving abstract science on one side, and being guided by observation only, it would appear that the depression which can be attributed to the centrifugal tendency does not exceed, and is perhaps rather less than one inch; for though there is reason to believe that the extreme depression is considerably more, we cannot overlook the fact that there are other forces in operation which must modify and in some instances increase the effect due to the rotation of the earth. Of these forces, one of the most important is that called into action by the winds which prevail in the southern hemisphere north of the antarctic circle. From the parallel of 60° northwards to the parallel of 40° these winds blow almost constantly from the west; they are not, as has been frequently said, north-west winds; neither are they south-west; but though found occasionally, or in some special locality generally, inclined the one way or the other, their prevailing direction right round the world is due west. These winds have a strength far beyond that of any other prevailing winds on the face of the earth; in

fact between the parallels of 45° and 55° it blows a gale of greater or less severity almost the whole year through. South of the parallel of 60° these winds appear to lose both their force and their regularity; and inside the antarctic circle the winds are very variable, and by no means excessive in strength.

It is thus quite impossible to admit that these winds are in any way caused by the low barometer to the southward of them. They do not blow towards it either directly or in a vorticoſe manner, but preserve their due westerly direction as long as they preserve their distinctive force, and even after they have in a great measure lost it. There is not a particle of evidence in support of any idea of a vorticoſe movement; and however the winds may be caused, and, so far as the present question is concerned, whether they are rightly named counter-trades or not, we may consider it clearly established that they are not caused by the low barometer of the Antarctic. And, indeed, if that low barometer is itself due to such a centrifugal tendency, it is impossible that it should cause any inward movement of the air. The spheroidal form which the atmospheric strata have been compelled to assume is the form of equilibrium; and that once taken, the centrifugal tendency is powerless to cause motion, save only when it ejects, and sometimes with violence, any excess of air which by some meteorological change has been driven southwards.

The condition of the atmosphere in high southern latitudes is therefore that of a large body of air in a state of comparative rest, past which a large and strong current is ceaselessly rushing. According to the nature of fluids under such a condition, the mass in motion tears away particles from the mass at rest, and establishes a constant tendency to rarefaction. This is the property of fluids first set forth by Daniel Bernoulli in his *Hydrodynamica*, where he shows by a series of experiments that, if one end of a pipe be fitted as a branch into another somewhat larger and opening freely, water running through the larger pipe will draw water up from a vessel into which the lower end of the smaller pipe has been inserted*. The friction of the water rushing through the main pipe against the end of the column of air in the smaller tears away the air bit by bit; and as water from the vessel below is forced up into the vacuum so formed, it too is torn off and carried away. If instead of leading into a closed vessel of water the branch lead into a closed vessel containing air, this air may in exactly the same manner be drawn out till its elastic force is exhausted; and in this way, by keeping a continuous stream of mercury running through the main pipe, the

* *Hydrodynamica, sive de viribus et motibus fluidorum commentarii*, 1738, p. 276.

nearest approach to a perfect vacuum may be obtained, as is in fact done by Sprengel's air-pump.

But though this principle of fluids has long been known and admitted, it seems to have been regarded rather as a scientific curiosity than as affecting the everyday relations of life. One familiar instance will show that such a view is very erroneous. Certain gratings in the streets cover shafts which lead directly to the main drains, and it might be supposed that through these foul smells would be very apt to escape into the open air; but the flow of the water and the air which it drags with it through the drain prevents this; it continually rasps off the bottom of the atmospheric column and causes a descending current of air through the shaft, so that no vapours from below can come up. The force of this descending current can easily be seen by holding a lighted torch near the grating of such a shaft and watching how the flame is sucked in—quite as strongly sometimes as it would be near the bottom of a doorway opening into a heated room; and indeed the power of friction, acting in a similar way, was tried a few years ago in the ventilation of coal-mines. A number of jets of steam were driven forcibly into the upcast shaft, and these, dragging the air with them, gave rise to a corresponding descending current in the downcast shaft. In practice, however, the method was found expensive, and has been gradually given up even by those who were at first very much in favour of it*.

From a meteorological point of view, the importance of this principle was, I believe, first brought forward by Sir Henry James†, who established the fact that in the heavy squalls of a gale of wind a barometer on the lee side of a wall had a lower reading, and one on the weather side a higher reading, than a third barometer placed without obstruction in open ground. More recently Professor Wild, of Bern, has shown that a southerly gale blowing over the Alps and passing as an upper current over the valleys of the north-eastern part of Switzerland, more especially of the cantons Uri, Glarus, and Schwytz, draws the air of the confined valleys away with it, so as to cause a very marked depression of the barometer—and that in consequence air from above, which in passing over the mountains has been drained of its moisture and warmed by the heat of condensation, descends as a dry hot wind, which is almost peculiar to these localities, and is there known as the Föhn‡.

* For a full discussion of the merits of this system see "Experiments on the relative value of the Furnace and the Steam-jet in the Ventilation of Coal-mines," communicated to the North of England Institute of Mining Engineers by Nicholas Wood, President of the Society. Newcastle, 1853.

† Trans. of the Roy. Soc. of Edin. vol. xx. p. 377.

‡ Dr. H. Wild, *Ueber Fohn und Eiszeit*, 1868, p. 30.

Although very much larger, and having boundaries of a very different nature, the Antarctic is as confined as the valleys of Switzerland; and the friction which is continually going on at its exterior limits between the strong westerly winds and the still air inside must produce, or tend to produce, the same effect. It is impossible to form any reliable estimate of the effect which is really produced, because we know neither the absolute depression of the barometer, nor how much of it is due to the centrifugal tendency established by the rotation of the earth; but since the Swiss observations in the localities I have referred to give 5 to 6 millimetres as the barometric depression which precedes and accompanies the setting in of the Föhn, we may consider that two-tenths of an inch is the most that can be attributed to a similar cause in the Antarctic.

Into the comparative vacuum thus formed we might expect a certain portion of the air from outside to flow; and some does seem to do so, and to form a series of curling eddies on the inner or southern margin of the west winds; but the influence which these exercise on the west winds themselves is exceedingly small and altogether imperceptible. As, then, the west winds blow incessantly, and incessantly abstract air from the Antarctic, and yet the pressure of air in the Antarctic remains nearly constant, it is clear that air must be continually, or at least frequently poured in to take the place of that which is carried away. We have no evidence as to where or how this supply is given; that offered by the northerly winds does not appear sufficient; and if on such a point, in the utter absence of any positive knowledge, conjecture may be permitted, I would say that it seems not impossible that, in consequence of the continual removal of air from the Antarctic, the atmospheric strata outside, at some unknown distance from the surface of the earth, are left so much above the strata of equal density inside that the eastward force is overcome by the pressure southwards, and that they flow over. A supply so given at the top of the column would clearly be regulated by the quantity of air previously withdrawn from the bottom; and when the great current of westerly wind has been blowing stronger and tearing away more air than usual, more air than usual will be poured in above; so that, after the strength of the westerly gale has abated, there will be an excess of air within the confined circle, which being compelled by the centrifugal tendency to move outwards, may show itself in some locality as a southerly gale.

The arctic regions differ from the antarctic in many ways, and essentially in this; that whilst the Antarctic is open, bounded only by the westerly winds sweeping over an unbroken sea, the Arctic is shut in by the continental shores which extend round almost its entire circumference. The westerly winds, again, of

the northern hemisphere are neither so strong nor so regular as those of the southern, so that probably but little air is drawn out by their friction; but, on the other hand, the formation of the land, the trend of the coast and of the mountain-ranges, forces a very great deal of air from the south into the arctic circle. This is notably the case on the west coast of Norway, along which the atmospheric current flows into the polar regions in exactly the same way as the great current of the ocean (the Gulf-stream) does. Consequent on the centrifugal tendency, this would naturally be ejected; and a great part of it is ejected in a succession of gales, rather than in a constant wind, which find a vent on the east coast of Greenland or through Kennedy Channel; but the openings for escape are small; and it would appear that the resistance offered by the sluggish air resting over the continents of Asia and America resists even a considerable pressure induced by the centrifugal tendency. The elastic force of the air in the Arctic is thus kept permanently greater than it would be if the area was open; and the barometer, although low, has not so great a depression as would seem due to the centrifugal tendency in such a high latitude.

There is, however, in the neighbourhood of Iceland a small area in which, especially in winter, the barometer stands markedly lower than it does on its southern, eastern, or western side. After what I have said as to the effect on the barometer of the westerly winds circling round the antarctic regions, it would be unnecessary to dwell on the depression in this very small area further than to point out that the westerly winds of the North Atlantic, the south-westerly and southerly winds of the coast of Norway, veering, before we lose sight of them, to south-east and reappearing on the coast of Greenland as north-easterly gales, form a circuit round Iceland which would sufficiently account for the existence of even a lower pressure in that neighbourhood. But Professor Mohn, Mr. Buchan, and several others consider that this circuit of winds is due to the low pressure in the centre, round which they blow in a vorticoose manner, the air being continually drawn in towards the place of comparative vacuum. If this is the case, what causes the low pressure? I have already referred at length to Mr. Buchan's opinion that the low pressure is due to the relative heat. I have shown that Iceland cannot be considered an area of relatively high temperature, that areas of relatively high temperature do not necessarily become areas of relatively low pressure, and that the low pressure in the neighbourhood of Iceland cannot be due to any such cause. On the other hand, the westerly winds of temperate latitudes are incontrovertible facts. There is here no question of their origin; for, extending, as they do, right round

the world, they certainly cannot be referred to the small patch of low pressure near Iceland. Whatever their cause, it is independent of this. But by the ordinary laws which regulate the motion of fluids, a current of air, such as that of the west winds which rage in the Atlantic through the winter, must, on striking against a barrier across its path, be thrown back on itself in a circling eddy; and it seems to me a necessary consequence of the coast-line and mountains of Scotland and Norway that a large eddy should be formed just where we find the circuit round Iceland. It is therefore to the westerly winds of the Atlantic that I refer this circuit; and it is to the circuit that I refer the low barometer near its centre.

But if winds circling round any area tend to constitute it an area of low pressure (and I have shown that by the laws of the friction of fluids they must do so), a very remarkable anomaly is observed in each of the large oceanic basins, where, in the centre of the circuit formed by the gradual veering of the trade-winds on one side and of the west winds on the other, there is an area of distinctly high pressure. In the North Atlantic this area has from the earliest times borne the name of the Horse Latitudes; but it differs in no material point from the similar areas in the other oceans. The point of difference between these and the other areas in which a low pressure results from the circling of the wind suggests itself at once. The areas of high pressure are areas of warm, almost tropical ocean; and without referring to any hygrometric observations, we know that in these areas calms are frequent, and that during the calms the air at a high temperature must be tending towards saturation. Since, then, a great part of the air supplied to these regions from the polar or colder side enters with a mean temperature of 50° or 55° F., and containing vapour of an elastic force of $\cdot 3$ of an inch at the most, and remains warming itself, expanding itself, and drawing moisture from the ocean at a mean temperature of 65° F. till the elastic force of its vapour is not less than $\cdot 6$ of an inch, we may fairly attribute the high pressure in these areas in a great measure to the increase in the elastic force of the vapour.

These several instances of barometric variation are in a great measure typical or representative; and considering them as such, I would sum up the analysis of them which I have attempted in a statement of the conclusions at which I arrive.

By reason of the centrifugal tendency caused by the rotation of the earth, the height of the atmospheric column varies with the latitude; and with it the height of the barometer, if not controlled by other agencies, also varies, changing gradually from about 29 inches near the poles to about 30.1 inches near

the equator. High and low pressure are therefore terms to be applied relatively rather than absolutely ; and the Arctic, with a mean pressure of 29·6, is in reality an area of high pressure ; the Doldrums, with a mean pressure of 29·9, an area of low.

Owing to the distribution of land and water and the great meteorological disturbances introduced by it, the isobars, in point of fact, differ very widely from such a theoretical view as would make them coincide with parallels of latitude ; but where one element decidedly preponderates, and where there are no geographical obstructions, as in the great Southern Ocean, they approach very markedly to this parallelism.

A ruling temperature, whether high or low, does not appear to exercise any noticeable influence on the pressure ; but a temperature that is changing produces a tendency which is often shown rather by its effect on atmospheric movement than by the sluggish barometer ; and this tendency is according to the ordinary laws of the expansion and contraction of gases, and quite in opposition to that which has been proposed by meteorologists : an increase of temperature tends to cause a high barometer and to give rise to an outward movement ; a decrease of temperature, on the contrary, tends to make the barometer fall and the air move inwards.

The permanent presence of aqueous vapour in the air has no sensible effect on the barometer ; but in confined districts where aqueous vapour is forming freely the barometer ranges high, and where there is much rain (that is, condensation) it ranges low.

Where the prevailing winds blow strongly into a corner barred by mountains, as in Siberia, or bifurcate against a line of coast, as against the coast of the Spanish peninsula or California, the barometer stands high in consequence of the mechanical compression of the air ; and where the prevailing winds circle round any area, or sweep past the opening of a confined district, the barometer stands low in consequence of the abstraction of the air by friction.

According to these conclusions, the mean barometric pressure at any place, or for any season, depends principally on geographical conditions, on latitude, on the relation between land and sea, on the trend of the coast-line, on the direction and height of mountain-ranges, and on the nature and characteristics of the prevailing winds. Since, however, these geographical conditions remain constant, or vary only with the season, they do not appear to account for the deviations from the mean pressure which the barometer is every day showing. But the conclusions which I have referred to these conditions are founded, not only on a vast mass of geographical evidence, but on the known

laws which govern the expansion, the contraction, the compression or the rarefaction of gases, and hold true wherever they may be properly applied; so that, descending from great to small, and from regular to irregular, we may fairly argue that agencies which we can show regularly produce certain results on the largest scales are capable also of producing similar effects, even when the scale is limited or their mode of action constrained or distorted.

[To be continued.]

XLV. *On Subaqueous Vision.* By R. E. DUDGEON, M.D.*

THE dioptrical apparatus of the human eye consists mainly of two transparent media:—(1) the aqueous and vitreous humours with nearly the same index of refraction, which differs inappreciably from that of water; (2) the crystalline lens, the mean index of refraction of which is variously stated to be from 1·383 to 1·455. We may leave out of consideration the cornea and the capsule of the crystalline lens, as, though the indices of refraction of these membranes differ from those of the other dioptrical parts of the eye, their respective surfaces being parallel prevents them having any material influence on the passage of rays of light to the retina.

The radius of curvature of the cornea, which bounds the anterior surface of the aqueous humour, is (according to Donders, 'Accommodation and Refraction of the Eye,' p. 67) 8 millims. (=·32 inch). The radius of curvature of the anterior surface of the crystalline lens (which forms the posterior concave surface of the aqueous humour) is, according to the same authority, 10 millims. (=·4 inch). The shape of the aqueous humour is consequently a meniscus; and as its thickness is 3 millims. (=·12 inch) and its index of refraction 1·336, its absolute principal focus, ascertained by Sir D. Brewster's rule (Optics, p. 23), will be 46 millims. (=1·8 inch).

The shape of the crystalline lens is a double-convex lens of unequal convexities, the anterior surface (when accommodated for distance, *i. e.* when the accommodation is at rest) having a radius of curvature of 10 millims. (=·4 inch), the posterior one of 6 millims. (=·24 inch). The thickness of the lens being 5 millims. (=·2 inch), and its mean index of refraction (taking Sir D. Brewster's calculation) 1·383, its principal focus in air will be 6 millims. (=·24 inch). But as it is suspended in a medium whose index of refraction is 1·336, the sine of the angle of incidence of the rays of light is to the sine of the angle of refraction

* Communicated by the Author.

as 1·383 to 1·336, and its focus in this situation will therefore be 24 millims. (nearly 1 inch). As, however, it receives the rays of light already deflected by the more convex anterior aqueous meniscus, its actual focus in the eye is 18 millims. ($=\cdot 7$ inch), or precisely the distance of the posterior surface of the crystalline lens from the retina in the axis of vision.

Listing (*Dioptrik des Auges* in Wagner's *Handwörterbuch der Physiologie*, quoted by Donders *loc. cit.*) makes the mean index of refraction of the crystalline 1·455; and this index Donders accepts as the true one; but a simple measurement according to Sir D. Brewster's rule will show this to be too high an estimate; and Donders himself (p. 62) admits that it may be too high.

In the foregoing calculations I have assumed the surfaces of the cornea and crystalline lens to be spherical; but their actual shapes, as determined by Chossat by careful measurements of the eyes of oxen (*Annales de Chimie et de Physique*, vol. x.), are not quite spherical. That of the cornea is an ellipsoid of revolution round the major axis, whilst those of the crystalline lens are ellipsoids of revolution round the minor axis of the ellipse. The effect of these shapes will be to make the central portion of the cornea more convex, and the central parts of the surfaces of the crystalline less convex than if they were segments of spheres. But for all practical purposes they may be regarded as spherical.

When the eye is immersed in fresh water, which has the same index of refraction as the aqueous humour, the latter ceases to refract the rays of light transmitted to it through the water, and the crystalline becomes the only refracting medium in the dioptric system of the eye. But, as has been stated, the crystalline suspended in a watery medium has a focus of 24 millims. (nearly 1 inch), consequently 6 millims. ($=\cdot 24$ inch) beyond the retina, and distinct vision is destroyed.

Obviously the restoration of distinct vision below water can only be effected by supplying the loss of the anterior aqueous meniscus of the eye by means of a lens of equal power. We have found the aqueous humour to be a lens whose principal focus is 46 millims. ($=1\cdot 8$ inch); so all we have to do is to place before our immersed eye a lens having a similar focus in water. A glass lens which has such a focus in air has a focus of upwards of 7 inches in water, because the sine of the angle of incidence of rays of light on a glass lens in water is to the sine of the angle of refraction as 1·500 to 1·336, in place of being as in air 1·500 to 1. So if we use glass we must take a lens whose focus is only $\cdot 45$ inch in air, which will have a focus of 1·8 inch in water, and thus supply the place of the lost lens of the aqueous humour. Practically we find this is so; and with

a pair of spectacles fitted with lenses of that power we can see near and distant objects when we are below the water (provided it is perfectly clear) as distinctly as we can with the naked eye in the air.

But such spectacles are attended with the obvious defect that, though they give perfect vision below the water, they completely destroy vision except for objects within half an inch distance above water; and the swimmer would find it extremely awkward to have to adjust his spectacles to his eyes every time he dived, and remove them whenever he came to the surface. Therefore, for practical purposes, it would be desirable to possess spectacles which, while giving perfect vision below water, would offer no impediment to vision in the air.

A consideration of the media (air and water) engaged in the case of the anterior lens of the eye lost by immersion led me to the solution of this problem.

By causing the rays of light to pass through the water compelled to assume a convex shape and transmitted through air before reaching the eye, a lens of almost any required power may be constructed. Our object would be attained by a reversed air-meniscus (concavo-convex lens) formed by segments of thin glass globes with radii of curvature corresponding to those of the aqueous humour. But such a lens would be troublesome to construct and too small for use; and the same object can be attained by employing air-lenses of larger dimensions. I found that two segments of a glass globe of somewhat less than 2 inches diameter, arranged with their concavities outwards and united round their edges by a ring so as to form a double concave air-lens, when immersed in water, forced the water into the convexity required for refracting the light to a focus of 1.8 inch. The convergence of the rays thus produced is equal to that caused by the aqueous-humour lens in air, and suffices to effect the shortening of the focus of the crystalline required to bring the image to the retina, thus supplying the place of the extinguished lens of the aqueous humour and restoring perfect vision below water.

A pair of spectacles fitted with lenses of this description does not materially interfere with vision in the air, and gives perfect vision when we plunge below the water. I have constructed air-lenses of similar power by arranging together segments of glass globes of different diameters, as $1\frac{1}{2}$ inch with $2\frac{1}{2}$ inches diameter, $1\frac{3}{4}$ inch with 2 inches, and so on. I likewise found that good vision below water is obtained by lenses of various powers ranging between $1\frac{1}{2}$ inch and 2 inches focal length, though probably the best vision for both near and distant objects below water is obtained by lenses of the power indicated above. With these, small type may be read easily and distant objects distinctly per-

ceived. The faculty of accommodation possessed by the eye accounts for this power of seeing distinctly through lenses differing slightly in focal length.

The principle of a concave air-lens in an aqueous medium can be applied to the construction of object-glasses of microscopes for examining objects in water. For this purpose it is best to imitate the construction of the object-glasses of ordinary microscopes, and make the lower surface of the air-lens plane while the upper is concave. There is hardly a limit to the magnifying-power of such glasses; but I do not suppose that a lens of greater power than $\frac{1}{4}$ of an inch focus will be required; and probably one of $\frac{1}{2}$ an inch focus, or even lower power, will suffice for what I believe they will chiefly be used for, namely the observation of the operations and organs of minute aquatic organisms.

XLVI. *On the Determination of the Height of the Atmosphere.*

By MAXWELL HALL, of Pembroke College, Cambridge*.

LET p and ρ be the pressure and density of the air at any point above the surface of the earth; let g be the force of gravity at this point, and r its distance from the centre of the earth. Let p_0 , ρ_0 , g_0 , and r_0 be their values at the point on the earth's surface vertically below the former, and let z be the distance between these two points.

Suppose for the present that the temperature is 0° C. throughout the atmosphere, and neglect the effect of moisture and of the increase of the centrifugal force due to z ; then, if in the equations

$$p = k\rho(1 + \alpha t), \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

$$\frac{dp}{dz} = -g\rho, \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

$$g = g_0 \left(1 + \frac{z}{r}\right)^{-2} \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

we put $t=0$, and $k=ag_0$, we shall get by integration

$$a \log_e \frac{p_0}{p} = \frac{z}{1 + \frac{z}{r_0}},$$

or, approximately,

$$\log_e \frac{p_0}{p} = \frac{z}{a} \left(1 - \frac{z}{r_0}\right), \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

whence

$$\frac{\rho}{\rho_0} = e^{-\frac{z}{a} \left(1 - \frac{z}{r_0}\right)}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

* Communicated by the Author.

The value of a is to be found from the equation $p_0 = ag_0\rho_0$, where p_0 is measured by means of the mercurial barometer. Since k is constant, a will vary with the latitude; but its mean value for the whole earth is about 4.94 miles.

Thus the whole pressure of the atmosphere is equal to the whole pressure of a homogeneous atmosphere whose density is ρ_0 and height a ; and in consequence a is termed the height of "the homogeneous atmosphere."

Let $r_0 = 3958.4$ miles, then a sphere of radius r_0 will be equal in volume to the earth, and therefore $\frac{4\pi}{3} \{ (a+r_0)^3 - r_0^3 \}$ will be the volume of the homogeneous atmosphere; and its weight will be $\frac{4\pi}{3} g_0 \rho_0 \{ (a+r_0)^3 - r_0^3 \}$, which must be carefully distinguished from its whole pressure on the earth's surface, which is evidently equal to $4\pi a g_0 \rho_0 r_0^2$.

The weight $\frac{4\pi}{3} g_0 \rho_0 \{ (a+r_0)^3 - r_0^3 \}$ is approximately equal to $4\pi a g_0 \rho_0 r_0^2 \left\{ 1 + \frac{a}{r_0} \right\}$; and if h be the height of the exterior surface of the atmosphere above the surface of the earth, we get

$$4\pi a g_0 \rho_0 r_0^2 \left\{ 1 + \frac{a}{r_0} \right\} = \int_{r_0}^{r_0+h} 4\pi g p r^2 dr, \quad . \quad . \quad (6)$$

an equation to determine h .

But $g r^2 = g_0 r_0^2$, and $\rho = \rho_0 \epsilon^{-\frac{z}{a} \left(1 - \frac{z}{r_0} \right)}$ from (5), and $r = z + r_0$; therefore equation (6) becomes

$$a \left\{ 1 + \frac{a}{r_0} \right\} = \int_0^h \epsilon^{-\frac{z}{a} \left(1 - \frac{z}{r_0} \right)} dz.$$

Again,

$$\epsilon^{-\frac{z}{a} \left(1 - \frac{z}{r_0} \right)} = \epsilon^{-\frac{z}{a}} \cdot \epsilon^{\frac{z^2}{ar_0}} = \epsilon^{-\frac{z}{a}} \left(1 + \frac{z^2}{ar_0} \right)$$

approximately; we can now integrate; and dividing by a ,

$$1 + \frac{a}{r_0} = 1 - \epsilon^{-\frac{h}{a}} + \frac{1}{ar_0} \{ 2a^2 - \epsilon^{-\frac{h}{a}} (h^2 + 2ah + 2a^2) \};$$

therefore

$$\frac{a}{r_0} = \epsilon^{-\frac{h}{a}} \left\{ 1 + \frac{h^2 + 2ah + 2a^2}{ar_0} \right\};$$

and taking logarithms, we get, finally,

$$\frac{h}{a} = \log_{\epsilon} \frac{r_0}{a} + \frac{h^2 + 2ah + 2a^2}{ar_0}, \quad . \quad . \quad . \quad (7)$$

which may easily be solved by approximation; the resulting value of h is 33.4 miles.

To consider the effect of temperature and moisture upon this result, suppose that the force of gravity is constant; then, since $\alpha = 0.003665$ when t is expressed in the Centigrade scale, we may safely neglect any terms whose coefficients are the second or higher powers of α ; and therefore from (1) and (2) we get, by eliminating ρ ,

$$\frac{a}{p} \cdot \frac{dp}{dz} = -(1 - \alpha t);$$

therefore

$$a \log_e p = C - z + \alpha \int_0^z t dz,$$

where C is some constant introduced by integration.

Now there is no fixed relation between t and z , so that we cannot integrate any further; but $\int_0^z t dz = z t_m$, if t_m be the mean temperature of the column of air whose height is z , and hence we get

$$a(1 + \alpha t_m) \log_e \frac{p_0}{p} = z;$$

and thus the temperature may be supposed to combine with the moisture in making a variable; for Professor Airy (On Sound and Atmospheric Vibrations) has shown that if the tension of the vapour be equal to mp , then the height of the homogeneous atmosphere will be $a \left(\frac{1+m}{1+\frac{5}{8}m} \right)$.

Thus our result will be corrected for temperature and moisture if in equation (7) for a we write

$$a(1 + \alpha t_m) \left(\frac{1+m}{1+\frac{5}{8}m} \right). \quad . \quad . \quad . \quad . \quad (8)$$

Now since t_m can never be accurately determined for any time and place, it follows that h cannot be accurately determined, and a more rigorous investigation would be practically useless; we proceed to obtain a probable value of t_m for these latitudes, or for the whole earth.

Sir J. Herschel has shown (Meteorology) that an equation,

$$t = \tau_0 + \tau_1 p + \tau_2 p^2,$$

exists between the temperature and pressure at any point in the atmosphere out of reach of local disturbing causes, in which τ_0 , τ_1 , and τ_2 are constants for only a short space of time, and which must therefore be determined by balloon-ascent when required. Their mean values for these latitudes, the temperature being measured in the Centigrade scale, and the pressure in inches of

mercury, are approximately

$$\tau_0 = -66.111,$$

$$\tau_1 = + 5.037,$$

$$\tau_2 = - 0.074,$$

the temperature being 18° C. at the surface of the earth*, and -66° C. at the confines of the atmosphere.

Now if we put $t = \phi(p)$, the mean value of t for a column of air extending from the surface up to a point where the pressure is p_1 is

$$\frac{\int_{p_1}^{p_0} \phi(p) dp}{p_0 - p_1},$$

and therefore $t_m = \frac{1}{p_0} \int_0^{p_0} \phi(p) dp$; and taking p_0 to be 30 inches,

$$t_m = -13^\circ \text{ C.}$$

Again, if the mean tension of the vapour be taken as 0.3 inch for the whole atmosphere, m will be 0.01; and the effect of temperature and moisture is to diminish the height of the homogeneous atmosphere by 0.22 mile. Substituting this corrected value of a in equation (7), we get, finally,

$$h = 32.1 \text{ miles.}$$

With regard to the free surface of the atmosphere, it is not necessary to suppose that the elasticity of the air there is destroyed by intense cold; it is sufficient to say that at the surface the elastic force is counterbalanced by the force of gravity.

We shall now compare this result with that obtained by observations on the twilight arc, which give the depression of the sun below the horizon when the last gleam of daylight disappears.

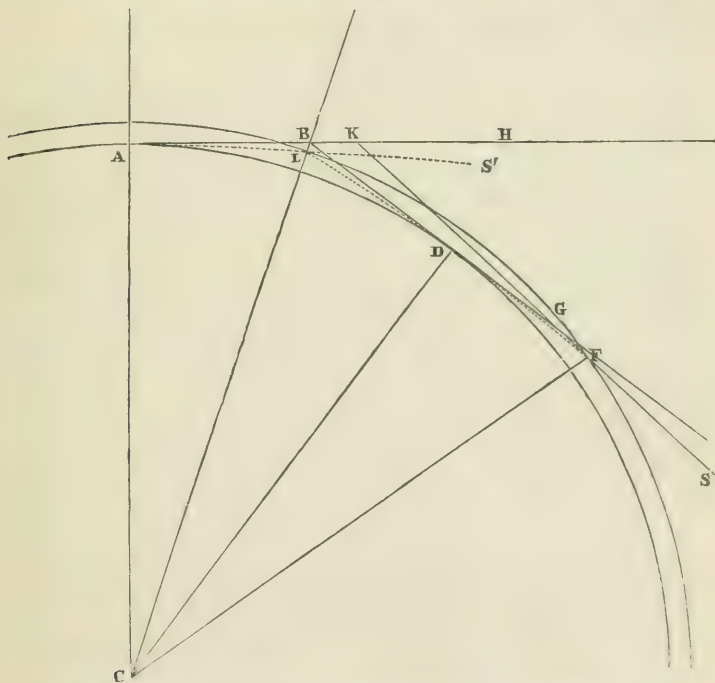
But this depression varies with the latitude and with the time of the year, and can never be accurately determined in consequence of the dispersion or scattering of the light due to the presence of vapour, or rather of particles of water suspended in the air, during its passage through the lower strata of the atmosphere.

The angle of depression is said to be from 16° to 17° in the tropics, and from 17° to 21° in higher latitudes (Chambers, 'Astronomy'); and since the presence of vapour dissolved in the atmosphere will tend to increase the refractive index of the air, and since vapour in the form of particles of water will scatter

* The mean temperature of the earth's surface is not the mean of the equatorial and polar temperatures. Brewster's formula, $t_F = 81.5 \cos \text{lat.}$, gives as the mean temperature 64° F., or 18° C.

the light, both these causes of irregularity will tend to *increase* the angle of depression, and it is obvious that we must employ the lowest value observed if we base our calculations upon the properties of dry air. We shall therefore suppose that the angle of depression is a little greater than 16° , with a probable error of about $\pm 30'$.

Let a ray of light from the sun, SF , be refracted so as to touch the circular section of the earth at D , and, refracted again to the same extent, cut the exterior circular section of the atmosphere at L . By reflection at L and by refraction this ray again touches the earth at A , causing the last gleam of twilight at A .



Let AH and BDG be tangents both to the ray and to the earth at the points A and D respectively; produce SF to K , cutting BDG in G .

Let θ be the angle $HK S$, the depression of the sun below the horizon; ϕ the angle of horizontal refraction $BG K$, and v the angle ACB . Then $2v = \angle HBD = \theta - \phi$, and therefore $v = \frac{1}{2}(\theta - \phi)$.

Let $S'L$ be the tangent to the ray at L , then all the circumstances attending the ray LA are the same as though the ray

proceeded from some object at S' ; let l_n be the angle BLS' , then (Main, 'Prac. Astron.')

$$\frac{\sin l_n}{\sin z} = \frac{r_0}{r_0 + h} \cdot \frac{\mu_0}{\mu_n},$$

where z , the apparent zenith distance, is in this case 90° , and μ_0 , μ_n the refractive indices of dry atmospheric air at the surface of the earth and at the highest point of the atmosphere respectively. The value of μ_0 is 1.000294, and that of μ_n is, of course, unity.

But

$$l_n - \phi = z - v;$$

therefore

$$l_n = 90^\circ + \phi - v = 90^\circ - \frac{1}{2}(\theta - 3\phi),$$

and therefore

$$r_0 + h = \mu_0 r_0 \sec \frac{1}{2}(\theta - 3\phi);$$

and if we take $\frac{1}{2}(\theta - 3\phi)$ to be $7^\circ 15' \pm 15'$, the resulting value of h is 33 miles ± 2 miles.

This result found by refraction is therefore in accordance with the value of h already found by means of the elastic properties of air; but, as we have already said, this method does not admit of any accuracy.

With regard to that medium whose existence is made apparent by shooting-stars, and by displays of the aurora borealis at great distances above the earth's surface, there is very little to be said at present. If it is a ponderable fluid of definite extent, it must be of a different nature to atmospheric air, and it will bear about the same relation to the atmosphere that the atmosphere itself does to the ocean surrounding the earth; but whatever may be its nature, it must be distinguished from the atmosphere proper, the subject of the above article.

XLVII. *On the Attraction of a Terminated Straight Line.*

By Professor CAYLEY, F.R.S.*

WRITE for shortness ($a, b, c; \epsilon$) to denote the shell included between the ellipsoids

$$\frac{x^2}{a^2} + \frac{y^2}{b^2} + \frac{z^2}{c^2} = 1 \text{ and } \frac{x^2}{a^2} + \frac{y^2}{b^2} + \frac{z^2}{c^2} = (1 + \epsilon)^2$$

(where ϵ is indefinitely small); then, if the ellipsoids

$$\frac{x^2}{a^2} + \frac{y^2}{b^2} + \frac{z^2}{c^2} = 1 \text{ and } \frac{x^2}{a'^2} + \frac{y^2}{b'^2} + \frac{z^2}{c'^2} = 1$$

are confocal, the attractions of the shells ($a, b, c; \epsilon$) and ($a', b', c'; \epsilon$) upon any exterior point P are proportional to their masses.

* Communicated by the Author.

Hence, considering a prolate spheroid of revolution, $c=b$, the attractions of the shell ($a, b, b; \epsilon$) will be proportional to those of the shell ($\sqrt{a^2-h}, \sqrt{b^2-h}, \sqrt{b^2-h}; \epsilon$); or if, as usual, $b^2=a^2(1-e^2)$, then, if h increases and becomes ultimately equal to b^2 , to those of the shell ($ae, 0, 0; \epsilon$); viz. this last is the portion of the axis of x included between the limits $x=-ae, x=+ae$; or say it is the terminated line $x=\pm ae$; and I say that the mass is distributed over this line *uniformly*.

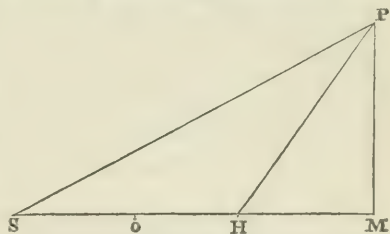
To see that this is so, observe in general that, in the spheroid $\frac{x^2}{a'^2} + \frac{y^2+z^2}{b'^2} = 1$, the volume included between the planes $x=\alpha$,

$x=\alpha+d\alpha$, is $=(y^2+z^2)d\alpha, =\pi\left(b'^2-\frac{b'^2}{a'^2}\alpha^2\right)d\alpha$; and thence,

writing $a'(1+\epsilon), b'(1+\epsilon)$ for a', b' , in the shell ($a', b', b'; \epsilon$) the volume included between the planes $x=\alpha, x=\alpha+d\alpha$ is $=\pi b'^2.2\epsilon d\alpha$; viz. this is independent of α , and simply proportional to $d\alpha$. Hence, writing $b'=0$, when the shell shrinks up into a line, the mass must be distributed uniformly over the line.

It follows that for a line of uniform density the equipotential surfaces are each of them a prolate spheroid of revolution having the extremities of the line for its foci, and that, if we have a shell bounded by any such surface and the consecutive *similar* surface, with its mass equal to that of the line, then such shell and the line will exert the

same attractions upon any point P exterior to the shell. The attractions of the line are obtained most easily by means of its potential; viz. taking S, H for the extremities of the line, and, as above, the



origin at the middle point, and the axis of x in the direction of the line, and writing $2ae$ for the length of the line, x, y, z for the coordinates of P, and r, s for the values of H P, S P (that is, $r=\sqrt{(x-ae)^2+y^2+z^2}, s=\sqrt{(x+ae)^2+y^2+z^2}$), then the potential is at once found to be

$$V=\log \frac{x+ae+s}{x-ae+r};$$

and we can hereby verify that the equipotential surface is in fact a spheroid of revolution having the foci S, H; for, taking the equation of such a spheroid to be

$$\frac{x^2}{a^2} + \frac{y^2+z^2}{a^2(1-e^2)} = 1,$$

(a is an arbitrary parameter, since only the value of ae has been defined), we have

$$s = a + ex, \quad r = a - ex;$$

and thence

$$x + ae + s = (1 + e)(x + a),$$

$$x - ae + r = (1 - e)(x + a),$$

and the quotient is $= \frac{1+e}{1-e}$, a constant value, as it should be.

The equation $V = \text{const.}$ may in fact be written

$$\frac{1+e}{1-e} = \frac{x+ae+s}{x-ae+r};$$

viz. this equation, apparently of the fourth order, breaks up into the twofold plane $y^2=0$, and the spheroid $\frac{x^2}{a^2} + \frac{y^2+z^2}{a^2(1-e^2)} = 1$.

The foregoing results in regard to the attraction of a line are not new. See Green's 'Essay on Electricity,' 1828, and Collected Works, Cambridge, 1871, p. 68; also Joachimsthal, "On the Attraction of a Straight Line," with Sir W. Thomson's Note, Camb. and Dub. Math. Journ. vol. iii. (1848) p. 93; but it does not appear to have been noticed that they are, in fact, included in the theory of the attraction of ellipsoids.

The like considerations show that the attractions of the ellipsoidal shell ($a, b, c; \epsilon$) upon an exterior point are equal to those of an elliptic disk $z=0$, $\frac{x^2}{a^2-c^2} + \frac{y^2}{b^2-c^2} = 1$, the mass of which is equal to that of the shell, and which has the density at the point (x, y) proportional to $\left(1 - \frac{x^2}{a^2-c^2} - \frac{y^2}{b^2-c^2}\right)^{-\frac{1}{2}}$.

Sir W. Thomson informs me that the foregoing results have long been familiar to him.

XLVIII. *On Fractional Criticoids.* By Sir JAMES COCKLE, F.R.S., Corresponding Member of the Literary and Philosophical Society of Manchester, President of the Queensland Philosophical Society, &c.*

1. **T**HERE is a conformity between the rational and entire algebraical expression (or, as we may call it for the sake of brevity, quotic) $\phi(x, y, \dots)$ and the corresponding differential expression (or, as we may call it, quotoid) $\phi\left(\frac{d}{dx}, \frac{d}{dy}, \dots\right)$. Coeffi-

* Communicated by the Rev. Robert Harley, F.R.S.

cients, whether variable or constant, in the latter are as different in their nature from the differential symbols to which they are attached as the coefficients in the former from their accompanying variables. Suppose that ϕ is of n dimensions, and that, in the quotic, x, y, \dots are respectively changed into $x + \xi, y + \eta, \dots$ and that ξ, η, \dots are so determined as to cause the coefficients of x^{n-1}, y^{n-1}, \dots , which must be distinguished from those of $xy^{n-1}, yx^{n-1}, \dots$, to vanish. Call this a critical transformation, and let the other coefficients, excepting those of $x^n, y^n, \dots x^{n-1}y, xy^{n-1}, \dots$ which are unchanged, be called, after the conditions of such evanescence are satisfied, critical functions. When all the corresponding critical functions of two quotics are equal, each to each, then, if the quotics are of the same degree, one quotic is a transformation of the other. If they are not of the same degree, then a transformation will render them uniform.

2. When there is only one variable, critical functions have, in the differential calculus, analogues discussed in my paper "On Criticoids" in the last March (1870) Number of this Journal*. These criticoids are obtained in consequence of the analogy between the linear transformation of a quotic in one variable and the factorial transformation of a quotoid in one dependent and one independent variable. Quotics with more than one variable correspond to linear partial quotoids with one dependent variable and as many independent variables as there are variables in the quotics, and whereof the orders are the same as the degrees of the quotics. In pursuing the analogy between the linear transformation of quotics and the factorial transformation of quotoids, I shall confine this paper to quotics of the second degree in two variables, and to the corresponding quotoid of the second order with one dependent and two independent variables.

3. Let, then,

$$\phi(x, y) = ax^2 + 2bxy + cy^2 + 2ex + 2fy + g,$$

and we have

$$\phi(x + \xi, y + \eta) = ax^2 + 2bxy + cy^2 + 2\lambda x + 2\mu y + \phi(\xi, \eta),$$

where

$$\left. \begin{aligned} \lambda &= a\xi + b\eta + e, \\ \mu &= b\xi + c\eta + f. \end{aligned} \right\} \dots \dots \dots (1)$$

Hence

$$\xi\lambda + \eta\mu = a\xi^2 + 2b\xi\eta + c\eta^2 + e\xi + f\eta;$$

and when, as in the critical case, λ and μ both vanish, the last three equations give on reduction,

* Phil. Mag. S. 4. vol. xxxix. No. 260. pp. 201-211.

$$\xi = \frac{bf-ce}{ac-b^2}, \quad \eta = \frac{be-af}{ac-b^2},$$

$$0 = \phi(\xi, \eta) - e\xi - f\eta - g. \quad (2)$$

Consequently

$$\phi(x + \xi, y + \eta) = ax^2 + 2bxy + cy^2 + e\xi + f\eta + g,$$

and the transformed quotic is therefore

$$ax^2 + 2bxy + cy^2 + \frac{e(bf-ce) + f(be-af)}{ac-b^2} + g;$$

whereof the absolute term, or critical function, may be equated with either side of the identity,

$$\frac{e(bf-ce) + f(be-af) + g(ac-b^2)}{ac-b^2} = \frac{2bef-af^2-ce^2}{ac-b^2} + g.$$

4. When the equations, whereof any two entail the third,

$$\frac{a}{b} = \frac{b}{c} = \frac{e}{f}, \quad (3)$$

are, one or more of them, satisfied, we have particular cases of the transformation. If all are satisfied, the given quotic may be written

$$\frac{1}{a}(ax+by)^2 + 2\frac{e}{a}(ax+by) + g.$$

In such a case μ is a multiple of λ , the equations $\lambda=0$ and $\mu=0$ are no longer independent, the quotic in substance involves only one variable, and the critical function is

$$\frac{1}{a}(a\xi+b\eta)^2 - 2\frac{e}{a}(a\xi+b\eta) + g.$$

If we use the symbol k as a general representation of critical functions, and reduce the formula last preceding by means of $\lambda=0$ or $\mu=0$, we have

$$k = g - \frac{e^2}{a},$$

which, in virtue of the relations

$$\frac{e^2}{a} = \frac{ef}{b} = \frac{f^2}{c},$$

may be written in various forms. When $ac-b^2=0$, and $e=0$ and $f=0$, we have a still more restricted quotic. When $ac-b^2=0$, and the other equations of the group (3) are not satisfied, then there is no critical transformation. When a , b , and e , or when b , c , and f vanish, we again have restricted quotics.

5. When $be-af=0$, and the other equations of the group (3)

are not satisfied*, we have

$$\xi = -\frac{e}{a}, \quad \eta = 0, \quad k = g - \frac{e^2}{a} = g - \frac{ef}{b};$$

and when $bf - ce = 0$, and the other equations of the group (3) are not satisfied, we have

$$\xi = 0, \quad \eta = -\frac{f}{c}, \quad k = g - \frac{f^2}{c} = g - \frac{ef}{b}.$$

In the first case we have $a\xi + e = 0$, or $b\xi + f = 0$; and in the second we have $c\eta + f = 0$, or $b\eta + e = 0$. All four forms are embraced in the group (1). When the general critical function becomes a vanishing fraction, we see that the limiting value of the fraction gives the particular critical function. For the comparison by the above process of the critical functions of quotics, it is of course necessary that the coefficients of the leading terms, or terms of highest degree, in one should be equal to, or the same multiple of, the corresponding coefficients in the other.

6. In order to pursue the analogy between quotics and quotooids, let

$$\psi(z) = ar + 2bs + ct + 2ep + 2fq + gz,$$

wherein p, q, r, s, t have the meaning usually assigned them in the theory of linear biordinals, and the coefficients a, b, c, e, f, g are in general functions of the independent variables x and y . Also let the differential coefficients of a certain auxiliary quantity ζ be represented as follows:

$$\frac{d^2\zeta}{dx^2} = \rho, \quad \frac{d^2\zeta}{dx dy} = \sigma, \quad \frac{d^2\zeta}{dy^2} = \tau, \quad \frac{d\zeta}{dx} = \xi, \quad \frac{d\zeta}{dy} = \eta;$$

and form the expression $\epsilon^{-\zeta}\psi(\epsilon^\zeta z)$; that is to say, substitute $\epsilon^\zeta z$ for z in the quotoid and divide the result by ϵ^ζ . The result, so divided, will be

$$ar + 2bs + ct + 2\lambda p + 2\mu q + \theta z,$$

* When $be - af = 0$, then

$$2bef - af^2 - ce^2 = (b^2 - ac)\frac{e^2}{a} = (b^2 - ac)\frac{ef}{b},$$

and

$$bf - ce = (b^2 - ac)\frac{e}{a} = (b^2 - ac)\frac{f}{b};$$

and when $bf - ce = 0$, then

$$2bef - af^2 - ce^2 = (b^2 - ac)\frac{f^2}{c} = (b^2 - ac)\frac{ef}{b},$$

and

$$be - af = (b^2 - ac)\frac{f}{c} = (b^2 - ac)\frac{e}{b}.$$

When both conditions are fulfilled, the values of ξ and η are more general than when one only holds.

wherein λ and μ have the respective values written opposite to them in the group (1), and

$$\theta = a(\rho + \xi^2) + 2b(\sigma + \xi\eta) + c(\tau + \eta^2) + 2e\xi + 2f\eta + g;$$

or, recurring to the notation of art. 3,

$$\theta = a\rho + 2b\sigma + c\tau + \phi(\xi, \eta).$$

In the critical transformation both λ and μ vanish. Hence, by (2),

$$\theta = a\rho + 2bs + c\tau + e\xi + f\eta + g. \quad . \quad . \quad . \quad (4)$$

Again,

$$\frac{d\lambda}{dx} = a\rho + b\sigma + \frac{da}{dx}\xi + \frac{db}{dx}\eta + \frac{de}{dx},$$

$$\frac{d\mu}{dy} = b\sigma + c\tau + \frac{db}{dy}\xi + \frac{dc}{dy}\eta + \frac{df}{dy}.$$

Consequently

$$\begin{aligned} \frac{d\lambda}{dx} + \frac{d\mu}{dy} = 0 = & a\rho + 2bs + c\tau \\ & + \left(\frac{da}{dx} + \frac{db}{dy}\right)\xi + \left(\frac{db}{dx} + \frac{dc}{dy}\right)\eta + \frac{de}{dx} + \frac{df}{dy}. \end{aligned}$$

Hence, substituting in (4),

$$\theta = \left(e - \frac{da}{dx} - \frac{db}{dy}\right)\xi + \left(f - \frac{db}{dx} - \frac{dc}{dy}\right)\eta + g - \frac{de}{dx} - \frac{df}{dy}; \quad . \quad (5)$$

and this will be the criticoid, provided that the values of ξ and η , given in art 3, satisfy the condition

$$\frac{d\xi}{dy} = \frac{d^2\zeta}{dy dx} = \frac{d^2\zeta}{dx dy} = \frac{d\eta}{dx}.$$

Since ζ is not, in general, obtained by the solution of a differential equation, it is necessary to test the desired transformation by the condition

$$\frac{d\xi}{dy} = \frac{d\eta}{dx}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

When this condition is satisfied we have, for determining ζ , the equation

$$\zeta = \int (\xi dx + \eta dy).$$

If (6) is not fulfilled the transformation cannot be effected.

7. In dealing thus far with the quotoid none of the conditions (3) are supposed to hold. Now, assume that $be - af = 0$ alone obtains. Then

$$\xi = -\frac{e}{a} = -\frac{f}{b}, \quad \eta = 0,$$

and, by (6), we must have

$$\frac{d}{dy}\left(\frac{e}{a}\right) = \frac{d}{dy}\left(\frac{f}{b}\right) = 0;$$

in other words the fraction $\frac{e}{a}$ or $\frac{f}{b}$ must be free from y . If this be the case, then

$$\zeta = -\int \frac{e}{a} dx = -\int \frac{f}{b} dx,$$

and the criticoid is

$$\theta = \left(\frac{da}{dx} + \frac{db}{dy} - e\right)\frac{e}{a} + g - \frac{de}{dx} - \frac{df}{dy}.$$

8. Next, of the group (3) let $bf - ce = 0$ alone be satisfied. Then

$$\xi = 0, \quad \eta = -\frac{f}{c} = -\frac{e}{b};$$

and, by (6),

$$\frac{d}{dx}\left(\frac{f}{c}\right) = \frac{d}{dx}\left(\frac{e}{b}\right) = 0;$$

that is to say, the fraction $\frac{f}{c}$ or $\frac{e}{b}$ must not contain y . If this be so, then the criticoid is

$$\theta = \left(\frac{db}{dx} + \frac{dc}{dy} - f\right)\frac{f}{c} + g - \frac{de}{dx} - \frac{df}{dy}$$

and

$$\zeta = -\int \frac{e}{b} dy = -\int \frac{f}{c} dy.$$

In this and in the preceding case the criticoid may be written in various forms, determined by the mode of elimination.

8. Let all the equations of the group (3) be satisfied. In this case multiply the given quotoid into a , and it becomes, after the elimination of c and f ,

$$a^2r + 2abs + b^2t + 2e(ap + bq) + agz;$$

and if we transform this last quotoid, and divide the result as before, we have the transformed and divided quotoid,

$$a^2r + 2abs + b^2t + 2\lambda(ap + bq)$$

$$+ \{a^2(\rho + \xi^2) + 2ab(\sigma + \xi\eta) + b^2(\tau + \eta^2) + 2e(a\xi + b\eta) + ag\}z = \chi(z),$$

wherein λ has the value written opposite to it in the group (1); that is to say,

$$\lambda = a\xi + b\eta + e.$$

Hence

$$(a\xi + b\eta)\lambda = a^2\xi^2 + 2ab\xi\eta + b^2\eta^2 + e(a\xi + b\eta) = 0,$$

since, in the critical case, λ vanishes. Therefore we may now write

$$\chi(z) = a^2 r + 2abs + b^2 t + \mathfrak{S}z,$$

where

$$\mathfrak{S} = a^2 \rho + 2ab\sigma + b^2 \tau + e(a\xi + b\eta) + ag. \quad . \quad . \quad (7)$$

But

$$\begin{aligned} \left(a \frac{d}{dx} + b \frac{d}{dy}\right) \lambda &= a^2 \rho + 2ab\sigma + b^2 \tau + \left(a \frac{da}{dx} + b \frac{da}{dy}\right) \xi \\ &+ \left(a \frac{db}{dx} + b \frac{db}{dy}\right) \eta + a \frac{de}{dx} + b \frac{de}{dy} = 0. \end{aligned}$$

Hence, by substitution in (7),

$$\mathfrak{S} = \left(ae - a \frac{da}{dx} - b \frac{da}{dy}\right) \xi + \left(be - a \frac{db}{dx} - b \frac{db}{dy}\right) \eta - a \frac{de}{dx} - b \frac{de}{dy} + ag.$$

And this is the criticoid, very general in its nature; for inasmuch as the two equations of the group (1) are now equivalent, one to the other, ξ is to be determined from the linear partial differential equation of the first order,

$$a \frac{d\xi}{dx} + b \frac{d\xi}{dy} + e = 0, \quad . \quad . \quad . \quad (8)$$

and the condition (6) will then be satisfied identically.

9. If the sum of the first two terms of \mathfrak{S} , viz.

$$\left(e - \frac{da}{dx} - \frac{b}{a} \frac{da}{dy}\right) a\xi + \left(e - \frac{a}{b} \frac{db}{dx} - \frac{db}{dy}\right) b\eta,$$

be proportional, term for term, to $a\xi + b\eta$, then

$$\frac{da}{dx} + \frac{b}{a} \frac{da}{dy} = \frac{db}{dy} + \frac{a}{b} \frac{db}{dx}; \quad . \quad . \quad . \quad (9)$$

and when (9) holds, then, in virtue of $\lambda = 0$, or of (8), the criticoid becomes, after reduction,

$$\mathfrak{S} = \left(\frac{da}{dx} + \frac{b}{a} \frac{da}{dy} - e\right)e - \left(\frac{de}{dx} + \frac{b}{a} \frac{de}{dy} - g\right)a, \quad . \quad . \quad (10)$$

and we have the paradox of a definite criticoid obtained by an arbitrary transformation. The explanation of this anomalous result is as follows.

10. Let D and Δ be symbolical operators defined thus,

$$D = a \frac{d}{dx} + b \frac{d}{dy}, \quad \Delta = a^2 \frac{d^2}{dx^2} + 2ab \frac{d^2}{dx dy} + b^2 \frac{d^2}{dy^2};$$

then, an operator operating upon all that follows,

$$D^2 = DD = \Delta + \left(a \frac{da}{dx} + b \frac{da}{dy}\right) \frac{d}{dx} + \left(a \frac{db}{dx} + b \frac{db}{dy}\right) \frac{d}{dy}.$$

Hence, when (9) holds,

$$D^2 = \Delta + \left(\frac{da}{dx} + \frac{b}{a} \frac{da}{dy} \right) D,$$

and the quotoid of art. 8 may be written in either of the forms,

$$\{\Delta + 2eD + ag\}z,$$

or

$$\left\{ D^2 + \left(2e - \frac{da}{dx} - \frac{b}{a} \frac{da}{dy} \right) D + ag \right\} z.$$

But this last result admits of the symbolical decomposition

$$(D + U)(D + u)z,$$

provided that U and u satisfy the equations

$$U + u = 2e - \frac{da}{dx} - \frac{b}{a} \frac{da}{dy},$$

$$Du + Uu = ag.$$

Substituting in the latter the value of U obtained from the former, transposing, and replacing D by its equivalent, we have, for determining u , the partial equation of the first order,

$$a \frac{du}{dx} + b \frac{du}{dy} = u^2 - \left(2e - \frac{da}{dx} - \frac{b}{a} \frac{da}{dy} \right) u + ag.$$

11. Next, let $A=0$ be any solution whatever of $b dx - a dy = 0$. Then, as we see by (8), the form of ζ will, in general, be $\zeta = \beta + B$, wherein β is a definite function of y and x , and B is an arbitrary function of A . Moreover $DA=0$, and therefore in general $DB=0$ and $(D+u)Bz = B(D+u)z$. Consequently the transformed and divided quotoid becomes successively

$$\begin{aligned} \epsilon^{-\beta-B}(D+U)(D+u)\epsilon^{\beta+B}z &= \epsilon^{-\beta-B}(D+U)\epsilon^B(D+u)\epsilon^\beta z \\ &= \epsilon^{-\beta}(D+U)(D+u)\epsilon^\beta z; \end{aligned}$$

and it is seen that it is only the definite portion β of ζ that acts effectively in the transformation. It may be noticed in passing, that since, if $(D+u)z=0$, then also $(D+u)Bz = B(D+u)z=0$, therefore the solution of the partial linear and homogeneous differential equation of the first order may be written in the form $\beta B + \beta_2 B_2$ whenever it admits of the form $\beta + \beta_2 B_2$. When the condition (9) is fulfilled, then, equating the quotoid to a function of x and y , we have a soluble differential equation. When (9) does not hold, arbitrary functions enter into the criticoid, and give a wide range of transformations of the quotoid last discussed.

12. Thus the general quotic, wherein the coefficients are constant, and the quotoid wherein the coefficients are variable

possess analogous properties, provided that in the latter the condition

$$\frac{d}{dy} \left(\frac{bf - ce}{ac - b^2} \right) = \frac{d}{dx} \left(\frac{be - af}{ac - b^2} \right) \quad . \quad . \quad . \quad (11)$$

is satisfied. When all the coefficients of the quotoid are constant, then (11) is satisfied, and the critical functions and the criticoids have the same forms and values. In other cases they, in general, are of different forms and values. When the group (3) holds both for quotic and quotoid, and the coefficients of the latter are constant, the critical and criticoidal functions are substantially identical. When such coefficients are variable, and a , b , and c are equal, then the critical and criticoidal functions have the same form, though not the same value. When (9) is satisfied, the criticoid is definite. When (9) is not satisfied, the criticoid contains an arbitrary function, which should be so assigned as to give the simplest or most useful transformations. The name fractional criticoids sufficiently marks the criticoids discussed in this paper.

“Oakwal,” near Brisbane, Queensland,
Australia, January 11, 1871.

XLIX. *On a Theory of the Mutual Action between Electrified and Magnetized Bodies.* By PROFESSOR CHALLIS, M.A., F.R.S., F.R.A.S.*

I HAVE given a theory of Electric Force in the Number of the Philosophical Magazine for October 1860, a theory of Galvanic Force in that for December 1860, and a theory of Magnetic Force in the Numbers for January and February 1861. The same theories are reproduced, with corrections and amplifications, in my work ‘On the Principles of Mathematics and Physics.’ It is known from experiment that these three kinds of physical force are distinctly separated from each other in respect to the conditions under which they are generated. For the generation of electric force it is necessary to employ friction; galvanic force has its origin in the chemical action between two dissimilar substances in contact, or in a tendency to such action; and magnetic force is due either to the natural condition of substances like the loadstone, or to artificial magnetization. The experimental evidence of the reality of these forces is derived, as in the case of the force of gravity, from the motions, and accelerations of motion, which they are observed to impress on visible or tangible substances.

Now, according to the above-mentioned theories, the three

* Communicated by the Author.

kinds of force, although originating in such different circumstances, are all *modes of action of steady currents of the ætherial medium*. This general result gives the means of accounting at once, on hydrodynamical principles, for the mutual action, observed by Oersted, between the rheophore of a galvanic circuit and a magnetized needle. (See art. 18 of the "Theory of Galvanic Force" in the Philosophical Magazine for December 1860, and 'Principles of Physics,' pp. 611 & 612.) But the same result points to a mutual action between an electrified body and a magnetized body. I have, in fact, indicated in art. 18 of the communication in the Philosophical Magazine for October 1860, that the ætherial currents which account for electric force and those which account for magnetic force are generated under like circumstances; that is, in both cases a gradation of the interior density of the substance gives rise, in consequence of the earth's motion through the æther, to secondary ætherial streams. There is, however, the difference that whereas the gradation of density is maintained in the electrified body by interior molecular action due to the abnormal state into which its superficial atoms are put either by friction or by induction, in the magnetized body the gradation of density results from interior molecular action which is independent of the state of the superficial atoms.

From the above statements it will appear that I had reason from theory to expect that, by the intervention of the electric and magnetic currents, there would be, between an electrified body and a magnetized body, a mutual action which might be experimentally recognized. Now, although I do not undertake to assert that no experimental evidence of such action existed, I may say that I did not succeed in finding any, and that I failed to discover in Faraday's experiments any criterion for settling the question. I did not, however, consider this to be a fatal objection to my theory of the physical forces, because at the same time that the theory lacked this confirmation, there appeared to be no experimental evidence of a directly contradictory kind.

But now, at length, I am able to appeal to appropriate experiments. Those made by Mr. Vincent, "On the Relations of Magnetism and Static Electricity," the details of which are given in the April Number of this Magazine, bear directly on the question raised by the theory. It seems that these experiments fully establish the fact of mutual action between electrified and magnetized bodies, and thus confirm the theory. My object in making this brief communication is fulfilled by pointing to this confirmation, which I think I may regard as an important corroboration of my general theory of the physical forces.

Cambridge, April 14, 1871.

L. *On the Capillary Phenomena of the common Surface of two Liquids.* By G. QUINCKE.

[Continued from p. 266.]

II. *Flat drops or bubbles coated with heterogeneous liquids.*

11. **I**F on a flat drop of a liquid 1 of large diameter $2r$, which is on a horizontal glass plane in a liquid 2, a small drop of another liquid, 3, is placed, and this small drop of liquid 3 spreads itself out on the common surface of liquids 1 and 2*, then it coats this with a thin layer or film of extremely small thickness. It may then be assumed that the form of the common surface of liquids 1 and 3 is the same as that of the common surface of liquids 2 and 3.

The equations of § 3 will still hold good under this supposition, if $\alpha_{13} + \alpha_{32}$ be introduced instead of the constant α_{12} . We have

$$\alpha = a^2 \cdot \frac{\sigma_1 - \sigma_2}{2} = \alpha_{13} + \alpha_{32}, \quad . \quad . \quad . \quad (5a)$$

$$K - k = a, \quad . \quad . \quad . \quad . \quad . \quad . \quad (8a)$$

$$K = a \sqrt{1 - \cos \omega}; \quad . \quad . \quad . \quad . \quad . \quad (9a)$$

and for the case in which the angle $\omega = 180^\circ$,

$$K = a \sqrt{2}. \quad . \quad . \quad . \quad . \quad . \quad (10a)$$

In the following Tables, analogously to the earlier notation (§ 8), we have

$$\left. \begin{aligned} \alpha &= (K - k)^2 \frac{\sigma_1 - \sigma_2}{2}, \\ \bar{\alpha} &= \frac{K^2}{2} \cdot \frac{\sigma_1 - \sigma_2}{2}. \end{aligned} \right\} . \quad . \quad . \quad . \quad (14a)$$

In this calculation the thickness of the layer of the liquid 3 is assumed to be less than the length (which can still be determined at K or k), consequently somewhat less than 0.01 millim. But, on the other hand, this thickness is also presupposed $> 2l$, greater than twice the distance at which the molecular forces of capillarity are still effective.

12. In the first place, air may be taken as liquid 1, an air-bubble being blown in a liquid 2 under a horizontal glass plane in the manner described in § 5. A small drop of substance 3 is introduced, by means of a tube bent at right angles, on the surface of the air-bubble in liquid 2, where it spreads itself out.

* As to the conditions on which this result depends, compare Section V. §§ 24-32.

In the equations of § 11, for this case we may suppose

$$\sigma_1 = 0, \quad \alpha_{13} = \alpha_3.$$

At the moment of the spreading out a sudden change in the form of the air-bubble is observed ; it becomes broader and flatter.

The arrangement of Table XI., which gives a summary of the observations, is the same as that of the former Tables ; the last column contains the value of $\alpha_3 + \alpha_{32}$, as it was found from the earlier observations on air-bubbles or drops of liquid in other liquids (Table X. § 10).

TABLE XI.—Flat Air-bubbles in Water ($\frac{\sigma_2}{2} = 0.5$).

No.	Substance 3.	$2r$.	K.	k .	$K - k$.	$K \sqrt{\frac{1}{2}}$.	α .	$\bar{\alpha}$.	α calc. = $\alpha_3 + \alpha_{32}$.
		mm.	mm.	mm.	mm.	mm.	mgs.	mgs.	mgs.
1.	Bisulphide of carbon	28.6	5.332	1.450	3.902	3.770	7.61	7.11	7.531
2.	Olive-oil.....	30.7	4.901	1.362	3.539	3.465	6.26	6.00	} 5.856
3.	"	100	4.762	1.362	3.400	3.367	5.78	5.67	
4.	Oil of turpentine ...	20.0	3.803	1.000	2.803	2.689	3.920	3.615	} 4.210
5.	"	20.8	3.808	0.939	2.869	2.693	4.115	3.625	
6.	Petroleum	28.6	5.310	1.558	3.752	3.755	7.035	7.05	7.037

Flat Air-bubbles in Olive-oil ($\frac{\sigma_2}{2} = 0.4568$).

1.	Absolute alcohol ...	?	3.770	0.923	2.847	2.666	3.204	2.808	} 2.825
2.	"	?	3.701	0.921	2.780	2.617	3.054	2.707	
3.	"	?	3.534	0.830	2.704	2.499	2.890	2.469	

The calculated values of $\alpha_3 + \alpha_{32}$ show an agreement with the observed values of α and $\bar{\alpha}$ which may be called a perfect confirmation of the theoretical considerations. The small deviation in $\bar{\alpha}$ for oil of turpentine shows that the angle ω was not 180° .

In the experiments on air-bubbles in olive-oil the alcohol very soon drove away the oil from the horizontal glass plane, the air-bubbles changed their form very considerably, the vertical meridional element k disappeared, the air-bubbles spread themselves to the edge of the horizontal glass plane (compare § 5) and vanished into the air.

It may here be observed that very thin layers of liquid 3, which showed Newton's rings on the air-bubbles, were quite sufficient to produce the change of form of the air-bubbles. When the traces of liquid 3 were even still less, so that they could hardly have been recognized in any other way, a very re-

markable, although smaller change of form of the air-bubble took place. The thickness of the layer of liquid 3 is in the last case less than $2l$, or double the distance at which the molecular forces of capillarity are still effective*.

The change of form is, of course, more remarkable the more α_2 and $\alpha = \alpha_3 + \alpha_{32}$ differ from one another—particularly striking therefore with oil of turpentine and olive-oil.

When flat drops of water or olive-oil are laid on a horizontal glass plate sprinkled with lycopodium-powder, they have the same form (inverted) as an air-bubble of equal volume in these liquids, and α_2 could be determined from $K - k$ in the same way as for air-bubbles. The same holds good for large drops of water which dew forms in the folds of certain leaves—cabbages for instance.

If on such flat drops of water a trace of olive-oil or oil of turpentine, or on a flat drop of olive-oil a trace of oil of turpentine is placed, then the drop immediately becomes flatter and broader; hence a change of form occurs as in air-bubbles.

Owing to the difficulty of obtaining flat drops of water and oil of large diameter on glass plates, I have abstained from making measurements of these changes in form.

13. If on the surface of flat drops of a liquid 1 in air a small quantity of a liquid 3 be placed, which spreads itself out on this surface, then in the equations (5a) to (14a) of § 11 we may suppose

$$\sigma_2 = 0, \quad \alpha_{32} = \alpha_3.$$

On flat drops of mercury in air small drops of water, olive-oil, and oil of turpentine were placed. As soon as these spread out on the surface of the mercury, the drops are seen to become flatter and broader.

In the following Table a few observations are collected:—

* I will take this opportunity of remarking that I committed an oversight in a communication on the magnitude of l (*Gött. Nachr.* 1869, p. 217, *Pogg. Ann.* vol. cxxxvii. p. 403, 1869), when I said that, according to the views of Plateau, a thin film of a liquid could not exist when its thickness became $< 2l$. The experiments which I cited (Plateau, "*Rech. Exp. &c.* 5^e sér.," *Mém. de Brux.* vol. xxxiii. p. 44, 1861) appeared to me to justify this assumption; whilst Plateau only supposes the capillary pressure of a curved film of the liquid of the thickness D dependent on the same when $D < 2l$: compare Plateau, "*Rech. Exp.* 2^e sér.," *Mém. de Brux.* vol. xvi. p. 35 (1847).

TABLE XII.

Flat drops of Mercury in Air ($\frac{\sigma_1}{2} = 6.771$).

No.	Substance 3.	2r.	K.	k.	K-k.	α .	α calc. = $\alpha_{13} + \alpha_3$.
		millims.	millims.	millims.	millims.	mgrms.	mgrms.
1.	Water	20	3.529	0.830	2.699	49.33	50.83
2.	Olive-oil.....	32	3.117	0.770	2.347	37.30	
3.	„	38.2	3.164	0.796	2.368	37.96	} 37.95
4.	„	45.5	3.125	0.755	2.370	38.03	
5.	Oil of turpentine.	20	2.891	0.811	2.080	29.30	} 28.57
6.	„	30.2	2.944	0.869	2.075	29.16	
7.	„	„	2.905	0.875	2.030	27.90	
8.	„	34	2.837	0.687	2.150	31.30	

The observed values of α agree with the values calculated from α_3 and α_{13} according to Table X. § 10, and thus confirm the theory.

It is to be observed, however, that all observations do not give this result, and that in water which is placed on mercury, α is often less, in oil of turpentine on mercury α is often found to be greater, than the theory would lead us to expect.

The reason of this deviation is chiefly to be sought in the adsorption of moisture, which is condensed from the atmosphere on the surface of the mercury as soon as the latter is formed. Since, for the liquids examined by me which could be present in the form of vapour in the atmosphere,

$$\alpha_1 > \alpha_{13} + \alpha_3,$$

whilst the adsorption was going on I observed a change in the form of the drop, K and K-k became smaller.

This condensation of vapour also takes place, as I have already* attempted to prove, on the surface of metallic solids and solid bodies generally, and in other physical phenomena (as, for example, in experiments on radiation) is an important source of error under circumstances in which Magnus† has designated it by the name of *Vaporhäsion*.

A layer of condensed air on the surface of the mercury may have a similar influence to that of a thin layer of liquid.

The decrease of the capillary constant is to be attributed to this condensation on the surface, which I have myself observed to take place on flat drops of mercury *in vacuo*, since even this

* Pogg. Ann. vol. cviii. p. 326 (1859).

† Ibid. vol. cxxx. p. 207 (1867).

vacuum, as I expressly observed at the time, contains vapours of grease*.

The condensation of vapour on the surface of mercury appears to go on with tolerable rapidity; and in spaces containing vapour of oil of turpentine, K and $K-k$ are found to be unusually small. In like manner the vapour of petroleum and even the grease from the hair and body have considerable influence on the form of the drops of mercury.

Thus it may easily happen that liquid 3 may be placed on a drop of mercury which is already coated with an extremely thin layer of a liquid 4; then α will be

$$= \alpha_{14} + \alpha_{43} + \alpha_3.$$

Now, accordingly as this value is greater or less than $\alpha_{13} + \alpha_3$, α may be found by observation greater or less than the theory of § 11 requires. As the value $\alpha_{13} + \alpha_3$ for mercury and water as substances 1 and 3 of Table X. § 10 is very great, but for mercury and oil of turpentine is very small, for water placed as substance 3 on the apparently pure surface of mercury α must be found too small, and for oil of turpentine as substance 3 too large. Besides, α_{14} and α_{43} depend on the greater or less thicknesses of the films of substance 4, which is often less than twice the radius of the sphere of action, and is completely beyond examination, so that the difference between the observed and the calculated values must turn out sometimes greater and sometimes less (compare § 27).

Experiment confirms all these conclusions, often in an undesirable manner, as the form of a drop of mercury in air is often changed by a trace of liquid which was by no means supposed to be in the atmosphere.

On placing petroleum upon flat drops of mercury I likewise found in four experiments α always greater than the theory (according to the determinations of Table X. for $\sigma_{13} + \alpha_3$) had led me to expect.

The influence of the vapour adsorbed or condensed on the free surface of the drops naturally manifests itself more plainly in air than in the air-bubbles described in the experiments of § 12, which are protected from impurities by the surrounding liquids.

14. Instead of placing a liquid 3 on the free surface of a liquid 1 or 2 bounded by air, it may be placed on the common surface of two liquids 1 and 2, which was presupposed as the most general case in the theoretical considerations of § 11.

On flat drops of olive-oil or bisulphide of carbon in water, which were obtained according to the method described in § 7, oil of turpentine was placed, and on flat drops of mercury in water olive-oil, oil of turpentine, or petroleum was placed.

* Pogg. *Ann.* vol. cv. pp. 33, 43 (1858).

TABLE XIII.—Flat drops of several Liquids on one another.

Flat drops of Olive-oil in Water ($\frac{\sigma_2 - \sigma_1}{2} = 0.432$)

No.	Substance 3.	2r.	K.	k.	K-k.	$K\sqrt{\frac{1}{2}}$.	α .	α .	α calc. = $\alpha_{13} + \alpha_{32}$.
1.	Oil of turpentine....	mm. 47.4	mm. 8.877	mm. 2.360	mm. 6.517	mm. 6.246	mgrs. 1.835	mgrs. 1.702	} 1.177
2.	"	36.6	8.972	2.570	6.402	6.345	1.770	1.731	

Flat drops of Bisulphide of Carbon in Water ($\frac{\sigma_1 - \sigma_2}{2} = 6.1343$).

3.	Oil of turpentine...	27.1	7.526	2.256	5.270	5.321	3.721	3.803	1.177
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Flat drops of Mercury in Water ($\frac{\sigma_1 - \sigma_2}{2} = 6.271$).

4.	Olive-oil	28.9	3.446	0.885	2.561	2.437	41.31	37.26	} 36.29
5.	Oil of turpentine...	30	3.192	0.984	2.208	2.257	30.58	31.96	
6.	"	30	3.276	1.010	2.266	2.316	32.21	33.65	} 27.72
7.	"	30	3.122	0.905	2.217	2.208	30.83	30.58	
8.	"	"	3.286	0.942	2.344	2.324	34.45	33.87	
9.	Petroleum	"	3.560	1.098	2.462	2.517	38.02	39.74	} 32.77
10.	"	36.1	3.501	0.849	2.652	2.476	44.10	38.44	

The foregoing collection of experiments do not show at first sight a satisfactory agreement of theory and experiment.

This want of agreement depends on the difficulty of properly taking into account the masses present at the point of section P of the common surfaces. For the effect of the superficial tension α_{23} at the common surface of liquids 2 and 3, only particles of those liquids were considered; the influence of the particles of liquid 1 which were at P was quite neglected. This error is greater the greater the mass of these particles of liquid and the greater the density of the liquid 1. In like manner with respect to the effect of the forces α_{12} and α_{31} , the minute quantity of liquids 3 and 2 respectively present in the point P was neglected.

This difficulty is so much the greater, since the density of the liquids 2 and 3 (which touch each other in the point P within a distance from that point which is less than the radius of the sphere of action) may easily be modified by the presence of a particle of liquid 3.

The particles of liquid on the surface possess quite different properties from those within the liquid, and certainly different properties according to the nature of the body by which

the liquid is bounded. Poisson* thinks that the liquid in the neighbourhood of the surface is condensed (*un état particulier de compression du liquide*); yet, under certain circumstances (for example at the free surface), I should rather assume† a negative condensation—that is, an expansion.

In the experiments of the foregoing paragraph, air as liquid 2, when near the point P, did not perceptibly modify the magnitude α_{13} ; for the observed and calculated values of the magnitudes denoted by α in Tables XI. and XII. agree very nearly.

If liquid 3 is of greater density, the active mass at the point P will possess a considerable influence, the active forces at the point P exert a greater pull than the numbers obtained by means of the observations of the foregoing Section express, the magnitude α found in these determinations is too large. This difference of theory and observation is particularly apparent with drops of mercury.

Oil of turpentine placed on bisulphide of carbon appears to be quickly dissolved by the latter, since the change of form of the drop of bisulphide of carbon in water effected by that means is but inconsiderable.

With olive-oil and oil of turpentine the mixture goes on more slowly, so that in this case the form of the drop of olive-oil is at first much more strongly changed, and it behaves as if it were coated with oil of turpentine.

The change of density in a liquid 2 or 3 close to the common point of contact P by the presence of a body 1 is rendered probable by other circumstances.

Experiment shows (compare § 18) that a body moistened with a liquid 2 firmly retains the particles of liquid on its surface, so that the adhering or adsorbed layer of liquid on the surface of a substance 1 is only with difficulty separated from it. At the same time substance 1 may be either a solid body or a liquid. I shall afterwards have occasion to draw attention several times to this behaviour of adsorbed liquid layers; and the adsorbed (condensed?) water-layer may have favoured the pollution or the mixing of the water placed on substance 3 in the experiments now described (§ 14).

In any case it is probable that by the spreading of liquid 3 on the common surface of liquids 1 and 2, in which by degrees all points of this surface became sectional points P of the three common liquid surfaces, the density of liquid 3 was modified in some other way than if the same liquid had spread itself on the free surface (bounded by air) of 1 or 2.

Without a knowledge of this change of density and of the

* *Mém. de l'Inst.* vol. ix. p. 78 (1826).

† *Pogg. Ann.* vol. cviii. p. 326 (1859).

influence of the same on the magnitude of the capillary constants or tension of the surface, no mathematical expression for the form of the drops of liquid in the experiments now described can be constructed.

I will, however, expressly observe that the liquids are not perfectly moveable, as was presupposed in the theoretical considerations, consequently the motion of the particles of liquid against one another is prevented by a certain friction. Hence it is possible that liquid 3 spread itself out on a very thin adsorbed layer of liquid 2 on the surface of liquid 1, and thereby occasioned the difference between theory and experience (compare §§ 18 and 27).

III. Capillary elevations in submerged tubes.

15. The best-known experiment on capillarity is very likely the elevation of liquids in tubes which, filled with air, are immersed in a liquid 1.

Liquid 1 then rises above the horizontal level of the liquid (that is, the open plane surface of liquid 1) to a mean height (compare § 4)

$$h = \frac{2}{\sigma} \frac{\alpha \cdot \cos \omega}{r},$$

in which α is the capillary constant of the open surface, σ the specific gravity of the liquid, ω the marginal angle, and r the radius of the tube.

The air which is above the capillary meniscus and the plane surface of liquid 1 can be replaced by a liquid 2, which also now covers the upper end of the vertical capillary tube. According to whether the angle $\omega_{12} < \text{or} > 90^\circ$, an elevation or a depression is observed of the meniscus forming the common limit of liquids 1 and 2 in the capillary tube, above or below the horizontal plane in which the liquids 1 and 2 outside the capillary tube touch one another in the wider vessel which contains them.

Although Laplace* has treated this case theoretically, yet, to my knowledge, no experiment with regard to it has hitherto been made.

According to the axioms given in § 1, the weight of liquid borne by the capillary meniscus of the common surface, as well as the periphery of the tube, must be multiplied by $\alpha_{12} \cos \omega_{12}$. If h_{12} denotes the mean elevation of the capillary meniscus above the plane limit of liquids 1 and 2 of specific gravities σ_1 and σ_2 respectively in a vertical tube of radius r , then we have

$$\left. \begin{aligned} (\sigma_1 - \sigma_2) h_{12} \cdot r^2 \pi &= 2r\pi \cdot \alpha_{12} \cdot \cos \omega_{12}, \\ h_{12} &= \frac{2}{\sigma_1 - \sigma_2} \cdot \frac{\alpha_{12} \cos \omega_{12}}{r} \end{aligned} \right\} \quad . \quad (15)$$

* *Supplément au liv. x. de la Méc. Cél., Œuvres*, vol. iv. p. 491.

If the tube is not perfectly cylindrical, then r is the radius of the tube at the place where the capillary meniscus of the common surface meets the sides of the capillary tube.

In the case in which $\omega_{12} > 90^\circ$, this mean elevation h_{12} may even be negative; there will then be a depression. For the case in which the angle is known (for example when $\omega_{12} = 0$ or 180°), the capillary constant α_{12} of the common surface of both liquids can be calculated from equation (15).

16. The experiments on *water* and oil of turpentine were made thus: into a glass cylinder 40 millims. in diameter and 250 millims. in height some water was poured as liquid 1, and upon this oil of turpentine was very carefully allowed to trickle down the side of the glass cylinder as liquid 2. As an elevation of the capillary meniscus was to be expected, the height chosen for the column of oil of turpentine was considerably greater than that of the layer of water.

By means of the blowpipe-lamp, glass threads of suitable thickness were made from thicker glass tubing purified as much as possible. In a capillary tube so prepared, open at both ends, I allowed a column of water to rise and then closed the upper end by melting it, so that the tube was nearly filled with water. The glass thread was passed through two caoutchouc rings and thus fastened to a strip of pure plate glass 300 millims. long and 10 millims. broad, which had a scale of millimetres engraved upon it. Towards the upper end of the glass thread and beneath the upper end of the column of water a mark was made with a file; the strip of plate glass and capillary tube were immersed in the glass cylinder, so that the file-mark was in the oil of turpentine and the lower end of the glass thread stood in the water. When the point of the glass thread was broken off at the file-mark under oil of turpentine, the water in the capillary tube sank, the oil of turpentine pressed upon it, and the meniscus concave above sank to a height h_{12} above the horizontal limiting layer of water and oil of turpentine, which was read off on the vertical millimetre-scale by a horizontal telescope. The glass tube was cut in the place where the capillary meniscus was found, and the inner diameter $2r$ of the section measured with microscope and ocular micrometer in the manner described in § 4.

Experiments Nos. 1 and 2 of Table XIV. were made in the manner described; No. 3 differed in the capillary tube being filled with oil of turpentine instead of water, with the upper end in oil of turpentine and the lower end immersed in water. When the upper end was broken off under oil of turpentine, the water rose in the capillary tube, which was moistened inside with oil of turpentine. Thus in the restoration of equilibrium the meniscus moved upwards in No. 3 in a capillary tube moistened with oil of

turpentine, and downwards in Nos. 1 and 2 in a tube moistened with water.

TABLE XIV.—Elevation in submerged capillary Tubes.

Water—Oil of Turpentine.

No.	2r.	h.	hr.	$\alpha_{12} \cos \omega_{12}$.	ω_{12} .
	millim.	millims.	sq. millims.	mgram.	°
1.	0.813	54.9	22.32	1.265	0
2.	0.557	79.9	22.24	1.260	0
3.	0.506	56.4	14.28	0.809	50 8

The arrangement of the foregoing Table speaks for itself. The last column contains the angle ω_{12} as it results from the numbers in the last column but one together with the known value of the constant (Table X. § 10),

$$\alpha_{12} = 1.177 \text{ milligramme,}$$

obtained from observation of the drops. For experiments Nos. 1 and 2 the angle $\omega_{12} = 0$, and the agreement of the numbers obtained according to the two methods is satisfactory. In No. 3 the angle was different and approached $47^\circ 2'$, which I had found from observation of the drops.

The difference of the angle is explained by the difference of the liquid layer which the capillary meniscus in its movement had to drive away, and is connected with the difference already discussed (§ 14), which is shown in the neighbourhood of the limiting surface by particles of the same liquid with different substances.

17. Similar experiments were made with *bisulphide of carbon* as liquid 1, and *water* as liquid 2. In this case, however, a depression of the capillary meniscus occurs; the convex side is turned up towards the water. Hence the thickness of the layer of bisulphide of carbon in the wide glass cylinder is correspondingly greater as that of the layer of water above it increases.

The capillary tube was filled with distilled water before the submersion. The experiments gave the following results:—

TABLE XV.—Elevation in submerged capillary Tubes.

Bisulphide of Carbon—Water.

No.	2r.	h.	hr.	$\alpha_{12} \cos \omega_{12}$.	ω_{12} .
	millims.	millims.	sq. millims.	mgrams.	°
1.	2.652	—25	—33.14	4.451	180
2.	0.964	—66.6	—32.12	4.314	180
3.	0.888	—68	—30.21	4.057	180
			Mean	4.274	

The mean value of the numbers of the fifth column agrees very nearly with the constant $\alpha_{12}=4\cdot256$ milligrammes, derived from observation of the drops (compare Table X. § 10). Thus the angle ω_{12} in the experiments on capillary tubes was nearly 180° , whilst according to the earlier method, from observation of drops on a glass plane, it was found to be 167° .

These experiments, as well as those of the foregoing paragraph, may be looked upon as a confirmation of the theoretical considerations.

18. If we observe the elevation in vertical capillary tubes, whose upper end opens into *olive-oil*, and lower end into *water*, then a depression or an elevation of the capillary meniscus below or above the common plane limiting surface of the two liquids will take place, according to whether the capillary tube before the submersion was filled with olive-oil or water, and consequently according to whether the sides of the tube were moistened with olive-oil or water. This circumstance appears to me worthy of notice, as it shows with what difficulty an adsorbed layer of liquid is expelled from a solid body. Water which only now comes into contact with the solid sides behaves quite differently from water which is already in contact with them; it is the same with regard to olive-oil.

TABLE XVI.—Elevation in submerged capillary Tubes.
Water—Olive-oil.

No.	2r.	h.	hr.	$\alpha_{12} \cos \omega_{12}$.	ω_{12} .
	millims.	millims.	sq. millims.	mgram.	
1.	2.612	—18	—23.51	—1.015	118 58
2.	2.304	34.1	39.29	1.697	35 57

The last column of the above Table contains the values of the angle ω_{12} , calculated on the assumption that $\alpha_{12}=2\cdot096$ milligrammes, as was found from observation of the drops.

With drops of olive-oil, which were placed under a glass plane moistened with water, I found the angle $\omega_{12}=17^\circ$ (§ 10), a value which, considering the uncertainty of the method used, does not strikingly differ from $35^\circ 57'$, found in the present analogous case.

IV. Elevations in capillary tubes of several liquids superposed.

19. A fourth method of observing capillary phenomena at the common limit of two liquids consists in placing on a liquid *u* in a capillary tube a second liquid *o*, and observing the common elevation of the two liquids.

This method, as has been already mentioned (§ 2), was used by Th. Young, Gay-Lussac, and Bède.

If the length of the upper column of liquid in the capillary tube be h , and if h_o denote the elevation of the common meniscus above the horizontal plane level or the surface of the lower liquid u outside the capillary tube, r_o the radius of the capillary tube at the place of the meniscus of the open surface (bounded by air) of the upper liquid, r_{ou} the radius for the meniscus of the common surface of the two liquids within the capillary tube, and the notation already adopted be retained, the weight of liquid raised above the horizontal level of liquid u is borne by the two menisci, and, according to equation (1), § 1, we have:—

$$-\frac{2\alpha_o \cos \omega_o}{r_o} - \frac{2\alpha_{ou} \cos \omega_{ou}}{r_{ou}} + h_o \sigma_o + h_u \sigma_u = 0,$$

$$\alpha_{ou} \cos \omega_{ou} = \frac{1}{2} \frac{r_{ou}}{r_o} [r_o (h_o \sigma_o + h_u \sigma_u) - 2\alpha_o \cos \omega_o]. \quad (16)$$

For the special case in which $r_o = r_{ou}$ this equation becomes

$$\left. \begin{aligned} \alpha_{ou} \cos \omega_{ou} &= \frac{r_o h_o \sigma_o + h_u \sigma_u - 2\alpha_o \cos \omega_o}{2} \\ &= \frac{\Sigma r h \sigma - 2\alpha_o \cos \omega_o}{2} \end{aligned} \right\} \quad (17)$$

It follows from this that the capillary constant of the common surface is determined when $\Sigma r h \sigma$, as well as α_o (the capillary constant of the surface of the upper liquid o) and the two angles ω_o and ω_{ou} are known. The first is in many cases $= 0^\circ$, or at least may be assumed to be known, since the magnitude $\alpha_o \cos \omega_o$ is determined from the elevation of liquid o in capillary tubes (compare § 4); but ω_{ou} is not known, and can only be supposed $= 0^\circ$ or 180° in a few cases, as we shall afterwards see.

Hence this method is not to be generally recommended for the determination of the capillary constant of the common surface of liquids. However, it has the advantage of being easily carried out, requires only small quantities of liquid, and admits of liquid 1 being above and liquid 2 below, or conversely, and σ_1 may be greater or less than σ_2 .

By means of this method it is immediately seen that the capillary constant α_u of the lower liquid (upon which alone, according to the theory of Poisson*, the weight of liquid borne by the capillarity depends) has no influence whatever, and that $\alpha_{ou} \cos \omega_{ou}$ and $\alpha_o \cos \omega_o$ alone determine it.

It may also be observed that h_o may be very small, provided it is $> 2l$ (than twice the radius of the sphere of action). Equation (17) then holds good completely, as the free surface of liquid o

* *Nouvelle Théorie de l'Action Capillaire*, p. 142 (1831).

and the common surface of liquids o and u are then to be found nearly in the same place of the capillary tube. h_o will then $=0$, and

$$\alpha_{ou} \cos \omega_{ou} + \alpha_o \cos \omega_o = r \cdot h_u \frac{\sigma_u}{2}. \quad . \quad . \quad . \quad (18)$$

For the case in which $\omega_{ou} = \omega_u$, the phenomenon would be the same as with the elevation of a liquid in a capillary tube, if the capillary constant α of the one liquid were replaced by

$$\alpha_{ou} + \alpha_o = \alpha.$$

A very thin layer of liquid 2 upon a meniscus may considerably modify and diminish the elevation of liquid 1 when

$$\alpha_{12} + \alpha_2 < \alpha_1.$$

If oil be placed on water, then, according to Table X. § 10,

$$\alpha_2 = 8.253 \text{ milligrammes, } \alpha_{12} + \alpha_2 = 5.856 \text{ milligrammes;}$$

thus a great depression of the liquid in the capillary tube must take place when the oil is poured on, as Thomas Young* was the first to observe.

20. I made the experiments in a manner similar to that described in §§ 4 and 16. Out of the purest thick glass tubing glass threads of a suitable thickness were drawn by help of a blowpipe-lamp. In the capillary tube thus prepared, open at both ends, I allowed a column of liquid o to rise somewhat higher than h was intended to be. The capillary tube with the upper end closed was passed through two caoutchouc rings and thus fastened to a strip of pure plate-glass from 100 to 200 millims. in length and 10 millims. in breadth, which had a scale of millimetres etched upon it. The glass thread forming the capillary tube was scratched by a knife at its lower end and broken off so that the bottom opening coincided with the zero-point of the scale, and thus the height h_o could be determined in the non-immersed part of the tube. This precaution was necessary in those cases in which the meniscus of the common surface of liquids o and u could not be observed by optical means.

When the lower end of the glass thread, together with the scale, was immersed in the liquid u , without the caoutchouc rings coming into contact with the liquid, and the upper closed end of the capillary tube broken off, then the liquid o rose in the tube, the liquid u followed, and h_o and h_u could be read off with a horizontal telescope on the vertical scale.

After that the fine glass tube was cut through in the place of the upper meniscus, and the inner diameter $2r_o$ of the sectional surface determined (in the way described in § 4) with a microscope and an ocular micrometer. I did not determine the dia-

* Young's Works, vol. i. p. 463 (1855). *Encycl. Brit.* "Cohesion," sect. 2 (1816).

meter $2r_{ou}$ in the experiments in question, since other sources of error, such as the different widths of the capillary tubes, and particularly the variableness of the angle ω in these experiments, have a much greater influence, and my chief object was to prove the dependence of the common mean elevation on the capillary constant α_o of the surface of the upper liquid.

For the same reason, in the observed magnitudes h_o and h_u , which always refer to the summit of the meniscus, the correction $\frac{r}{3}$, or a corresponding value, has been omitted. In the calculation of the constants $\alpha_{ou} \cos \omega_{ou}$ according to equation (16), the magnitudes of the capillary constant α_o were used instead of $\alpha_o \cos \omega_o$, as I had found them from the elevations in capillary tubes (Table II. § 4).

In the following Table the observations on *bisulphide of carbon* and *water* are collected. In the first series of experiments the specifically lighter water was above; the meniscus of the common surface turned its convex side upwards. In the second series the specifically heavier bisulphide of carbon was above; the meniscus of the common surface had its concave side turned upwards. In the first series of experiments the meniscus of the common surface rose in a tube which was moistened inside with water, and in the second series in one moistened with bisulphide of carbon.

TABLE XVII.—Elevations in Capillary Tubes. Two Liquids.

Water above; Bisulphide of Carbon below.

$$\sigma_o = 1,$$

$$\sigma_u = 1.2687,$$

$$2\alpha_o = 14.47 \text{ mgrms.}, \quad \alpha_{ou} = 4.256 \text{ mgrms.}$$

No.	$2r_o$.	h_o .	h_u .	$\Sigma rh\sigma$.	$\alpha_{ou} \cos \omega_{ou}$.	θ_{ou} .
	millim.	millims.	millims.	mgrms.	mgrms.	
1.	0.316	0.6	62.4	12.62	-0.92	77 21
2.	0.306	1.4	43.4	8.62	-2.92	46 35
3.	0.268	7.2	35.2	6.95	-3.76	27 56
4.	0.259	22.8	31.8	8.17	-3.15	42 15
5.	0.276	34.9	21.1	8.50	-2.99	45 28
6.	0.093	112.3	35.2	7.27	-3.60	32 14

Bisulphide of Carbon above; Water below.

$$\sigma_o = 1.2687,$$

$$\sigma_u = 1,$$

$$2\alpha_o = 6.65 \text{ mgrms.}, \quad \alpha_{ou} = 4.256 \text{ mgrms.}$$

1.	0.325	1.8	86.2	14.37	3.84	25 33
2.	0.435	2.0	55.5	12.63	2.97	45 44
3.	0.325	21.0	64.9	14.87	4.09	16 4
4.	0.247	63.8	4.4	15.42	4.36	0

The column headed $\alpha_{ou} \cos \omega_{ou}$ shows that the angle ω_{ou} was very small or 0° only in experiments 3 and 4 of the second series when based on the value 4.256 milligrammes for α_{ou} formed from observation of the drops (Table X. § 10). Since the liquids become mixed in the course of time, and the angle ω changes with the time, as was evident from observations with flat drops, the different values of the angle ω_{ou} in the various experiments are not surprising, since, both in this case and in the experiments of Section III. (§§ 16 to 18), the friction of the liquid and capillary meniscus in the capillary tube materially influence the elevation h_u , to which the lower liquid rises above the common level, and make it too small.

The form of the meniscus and of the angle ω_{ou} of the common capillary surface appeared to me also to depend on the rapidity with which the liquids rose in the capillary tubes. This rapidity is itself dependent on the length and breadth of the submerged capillary tube, and on that part of it which is filled with air.

By shifting the capillary tube in the caoutchouc rings the meniscus was brought into other places of the tube, but without producing any material influence on the magnitude of h_u .

21. A series of similar experiments was made on *water* and *olive-oil*, *oil of turpentine*, or *petroleum*, all of which liquids have smaller capillary constants and lower specific gravities than water.

The meniscus of the common surface turns the concave side upward when oil is above and water below, and the convex side upward in the converse case.

Above each series of experiments is given that value of α_{ou} for the common surface of the liquids o and u , which was obtained from the observations on flat drops (Table X. § 10), and by means of which the angle in the last column above ($\theta_{ou} = 180^\circ - \omega_{ou}$) was derived from the numbers in the penultimate column.

TABLE XVIII.—Elevations in Capillary Tubes. Two Liquids.

Petroleum above; Water below.

$$\sigma_o = 0.7977, \quad \sigma_u = 1,$$

$$2\alpha_o = 5.132 \text{ mgrms.} \quad \alpha_{ou} = 3.834 \text{ mgrms.}$$

No.	$2r_o$.	h_o .	h_u .	$\Sigma r h \sigma$.	$\alpha_{ou} \cos \omega_{ou}$.	ω_{ou} .
	millim.	millims.	millims.	mgrms.	mgrms.	$^\circ$ $'$
1.	0.245	6.9	69.2	9.158	2.013	58 31
2.	0.208	14.5	45.2	5.914	0.391	84 9
3.	0.227	15.5	39.5	5.887	0.377	84 21

Water above; Petroleum below.

$$\sigma_o = 1, \quad \sigma_u = 0.7977, \\ 2\alpha_o = 14.47 \text{ mgrms.}, \quad \alpha_{ou} = 3.834 \text{ mgrms.}$$

No.	$2r_o$.	h_o .	h_u .	$\Sigma r h \sigma$.	$\alpha_{ou} \cos \omega_{ou}$.	θ_{ou} .
	millim.	millims.	millims.	mgrms.	mgrm.	°
1.	0.524	16.3	46.8	14.06	-0.205	86 56
2.	0.230	34.5	76.3	10.98	-1.745	62 56
3.	0.174	71.7	78.3	11.66	-1.405	68 31

Oil of Turpentine above; Water below.

$$\sigma_o = 0.8867, \quad \sigma_u = 1, \\ 2\alpha_o = 5.530 \text{ mgrms.}, \quad \alpha_{ou} = 1.177 \text{ mgrm.}$$

					ω_{ou} .	
1.	0.400	0.5	33.9	6.835	0.661	55 47
2.	0.495	3.8	25.9	7.249	0.860	46 55
3.	0.335	5.3	36.9	6.968	0.719	52 21

Water above; Oil of Turpentine below.

$$\sigma_o = 1, \quad \sigma_u = 0.8867, \\ 2\alpha_o = 14.47 \text{ mgrms.}, \quad \alpha_{ou} = 1.177 \text{ mgrm.}$$

						θ_{ou} .
1.	0.669	0	18.6	5.52	-4.475	
2.	0.529	1.1	38.6	9.06	-2.705	
3.	0.529	2.3	35.6	8.96	-2.755	
4.	0.586	5.2	36.9	11.11	-1.68	0
5.	0.339	10.7	66.8	11.84	-1.31	0
6.	0.677	11.8	27.1	12.13	-1.17	0
7.	0.744	15.7	18.9	12.07	-1.20	0
8.	0.357	17.4	62.5	12.98	-0.74	51 3

Olive-oil above; Water below.

$$\sigma_o = 0.9136, \quad \sigma_u = 1, \\ 2\alpha_o = 6.542 \text{ mgrms.}, \quad \alpha_{ou} = 2.096 \text{ mgrms.}$$

1.	0.416	7.8	22.8	6.22	-0.15	85 38
2.	0.451	8.1	18.8	5.21	-0.32	81 20

Water above; Olive-oil below.

$$\sigma_o = 1, \quad \sigma_u = 0.9136, \\ 2\alpha_o = 14.47 \text{ mgrms.}, \quad \alpha_{ou} = 2.096 \text{ mgrms.}$$

1.	0.444	9.4	56	13.44	-0.51	75 46
2.	0.301	23.4	74.6	13.79	-0.34	80 40
3.	0.268	28.4	86.3	14.38	-0.05	77 36

Alcohol above; Olive-oil below.

$$\sigma_o = 0.7906, \quad \sigma_u = 0.9136,$$

$$2\alpha_o = 4.476 \text{ mgrms.}, \quad \alpha_{ou} = 0.226 \text{ mgrm.}$$

No.	$2r_o$.	h_o .	h_u .	$\Sigma rh\sigma$.	$\alpha_{ou} \cos \omega_{ou}$
	millim.	millims.	millims.	mgrms.	mgrm.
1.	0.363	10	18.1	4.436	-0.020
2.	0.330	15	16.2	4.395	-0.040
Olive-oil above; Alcohol below.					
$\sigma_o = 0.9136, \quad \sigma_u = 0.7906,$					
$2\alpha_o = 6.341 \text{ mgrms.}, \quad \alpha_{ou} = 0.226 \text{ mgrm.}$					
1.	0.582	10	13.5	5.77	-0.28
2.	0.346	15	27.9	6.19	-0.07

The observations show that the angle ω_{ou} was not constant in the different experiments, with the exception of the series water above, oil of turpentine below—in which a value α_{ou} is found for Nos. 4 to 7, which very nearly agrees with the determination from observations of drops (Table X. § 10) when the angle ω_{ou} is put $= 180^\circ$. For the measurements 1 to 3 of the same series, the too small value of $\Sigma rh\sigma$ or $\alpha_{ou} \cos \omega_{ou}$ is owing to the very thin layer of water which covers the oil of turpentine. The oil of turpentine at the capillary meniscus of the common surface, freed from the water, being specifically lighter, rose in the column of water, spread itself out (compare § 26), and rendered impure the upper meniscus of the open surface of the water, which no longer possessed the constant for water, but that of a mixture of water and oil of turpentine. In observation No. 1 the water above the oil of turpentine was removed by a piece of blotting-paper.

In the series of experiments with olive-oil above and water below, the common surface was decidedly concave upward, whilst in contradiction herewith $\omega_{ou} > 90^\circ$ would follow from the observed elevations. Hence the cause that too small a value of h_u was observed must very likely be sought in the friction of the liquids against the sides of the tube, which very considerably retarded the condition of equilibrium.

The same also holds good for the experiments with alcohol and olive-oil given at the end of Table XVIII., where, on account of the smallness of α_{ou} , the fluctuations are perhaps still more striking.

22. I have also made a series of experiments with some liquids which mix in all proportions, in which therefore $\alpha_{ou} = 0$, and the weight of liquid raised above the common level of the

lower liquid depends only on the meniscus of the free surface or the constant α_o .

TABLE XIX.—Elevations in Capillary Tubes. Two Liquids.

Oil of turpentine above ; Olive-oil below.

$$\begin{aligned}\sigma_o &= 0.8867, & \sigma_u &= 0.9136, \\ 2\alpha_o &= 5.530 \text{ mgrms.}, & \alpha_{ou} &= 0\end{aligned}$$

No.	$2r_o$.	h_o .	h_u .	$\Sigma rh\sigma$.
	millim.	millims.	millims.	mgrms.
1.	0.549	10	12.6	5.602
2.	0.585	15	6	5.495
3.	0.222	20	34	5.405
Mean				5.601

Olive-oil above ; Oil of Turpentine below.

$$\begin{aligned}\sigma_o &= 0.9136, & \sigma_u &= 0.8867, \\ 2\alpha_o &= 6.542 \text{ mgrms.}, & \alpha_{ou} &= 0.\end{aligned}$$

1.	0.610	10	10	5.489
2.	0.316	15	24.3	5.574
3.	0.171	20	50.8	5.420
Mean				5.494

Alcohol above ; Water below.

$$\begin{aligned}\sigma_o &= 0.7906, & \sigma_u &= 1, \\ 2\alpha_{ou} &= 4.476 \text{ mgrms.}, & \alpha_{ou} &= 0.\end{aligned}$$

1.	0.509	10	11.4	4.915*
2.	0.396	5	49.5	10.590*
3.	0.301	20	13.6	4.421
4.	0.200	20	29.1	4.489
Mean				4.455

Water above ; Alcohol below.

$$\begin{aligned}\sigma_o &= 1, & \sigma_u &= 0.7906, \\ 2\alpha &= 14.47 \text{ mgrms.}, & \alpha_{ou} &= 0.\end{aligned}$$

1.	0.719	20	32.7	7.656
2.	0.680	15	2.3	5.719
3.	0.585	15	3.6	5.218
4.	0.331	10	81.4	12.31
5.	0.179	15	150	11.94
6.	0.158	20	198	13.96

In fact the foregoing collection of experiments show a value for $\Sigma rh\sigma$ which (with few exceptions, to which I shall presently return) very nearly agrees with the value of the constant $2\alpha_o$ derived from the elevations in capillary tubes (§ 4), as results from equation (16) when α_{ow} is put equal to zero.

In the series with olive-oil above and oil of turpentine below, after breaking off the point of the capillary tube I observed no movement for a long time; then a movement occurred suddenly, and the liquid certainly moved as quickly as if the oil of turpentine alone had ascended. The oil of turpentine, for instance, drove away the olive-oil from the glass side; and as soon as it had covered the meniscus of the olive-oil, it ascended, taking the column of olive-oil along with it. By this it is proved that $\Sigma rh\sigma$ in this case is found considerably less than $2\alpha_o$ for olive-oil, but, on the other hand, as nearly agrees with $2\alpha_o$ for oil of turpentine as might be expected generally in these experiments.

For "water above and alcohol below," only the mean of the last two experiments is taken, as, in Nos. 1 and 2, at the small height h_o the water diffused along the side of the tube had increased the quantity of water contained in the alcohol and with it $2\alpha_o$.

For "water above and alcohol below," the upper meniscus of the water in all the experiments is rendered impure by the rapid diffusion or solution of the specifically lighter alcohol and its ascent in the specifically heavy column of water, and possesses then naturally a smaller capillary constant than $2\alpha_o = 14.47$ milligrammes for water. According to the varying rapidity with which this contamination took place, the numbers of column $\Sigma rh\sigma$ vary in magnitude; the greatest is that in the last experiment, in which a small air-bubble between the columns of water and alcohol had retarded the diffusion of the alcohol in the water.

23. Results similar to those here described were obtained by Bède* in the experiments which he made by a similar method, and which here follow, with the notation which I have adopted in this memoir. The capillary constants, as determined by him, of the free surfaces of the liquids stand at the head of each series of experiments.

* *Mém. Cour. Brux.* vol. xxx. p. 187 (1860).

TABLE XX.—Elevations in Capillary Tubes. Two Liquids.

Petroleum above; Water below.

$$\begin{aligned}\sigma_o &= 0.791, & \sigma_u &= 1, \\ 2\alpha_o &= 5.220 \text{ mgrms.}, & 2\alpha_u &= 15.068 \text{ mgrms.}\end{aligned}$$

No.	r.	$\Sigma rh\sigma$.	$\alpha_{ou} \cos \omega_{ou}$.
	millim.	mgrms.	mgrm.
1.	0.955	6.397	0.588
2.	0.613	6.681	0.730
3.	0.343	6.810	0.795
4.	0.204	6.976	0.878
5.	0.160	6.831	0.805
		Mean.....	0.7595

Petroleum above; Sulphuric acid below.

$$\begin{aligned}\sigma_o &= 0.791, & \sigma_u &= 1.831, \\ 2\alpha_o &= 5.220 \text{ mgrms.}, & 2\alpha_u &= 11.542 \text{ mgrms.}\end{aligned}$$

1.	0.955	2.483	-1.368
2.	0.613	2.820	-1.200
3.	0.343	2.641	-1.289
4.	0.204	3.080	-1.070
5.	0.160	2.800	-1.210
6.	0.094	3.149	-1.035
		Mean	-1.195

Water above; Chloroform below.

$$\begin{aligned}\sigma_o &= 1, & \sigma_u &= 1.497, \\ 2\alpha_o &= 15.068 \text{ mgrms.}, & (2\alpha_u &= 5.724 \text{ mgrms.}).\end{aligned}$$

1.	0.843	10.15	-2.459
2.	0.094	5.57	-4.746
		Mean	-3.652

Analogous to the results of my experiments (§ 21), in those made by Bède the meniscus of the common surface of petroleum and water also turned its concave side, whilst that of the common surface of petroleum and sulphuric acid or water and chloroform turned its convex side to the specifically lighter liquid above it.

Since the angle ω_{ou} is unknown, the value of the capillary constant α_{ou} cannot be deduced from these experiments, as that angle (chloroform and water perhaps excepted) must have been between 0° and 180° .

Gay-Lussac* observed the depression of mercury in a tube wetted with water or aqueous alcohol, and found the following results, which, with the values of $\alpha_{ou} \cos \omega_{ou}$ calculated according to equation (17), § 19, are collected in the following Table. The values of α_o given therein were calculated from measurements made by Gay-Lussac†.

TABLE XXI.—Elevations in Capillary Tubes. Two Liquids.

Water; Mercury.

$$\begin{aligned} \sigma_o &= 1, & \sigma_u &= 13.6, \\ 2\alpha_o &= 15.13 \text{ mgrms.}, & (\alpha_{ou} &= 42.58 \text{ mgrms.}). \end{aligned}$$

$2r.$	$h_o.$	$h_u.$	$\Sigma r h \sigma.$	$\alpha_{ou} \cos \omega_{ou}.$	$\theta_{ou}.$
millim. 0.6472	millims. 7.730	millims. -7.415	mgrms. -60.23	mgrms. -37.68	$27^\circ 15'$
Alcohol; Mercury.					
$\begin{aligned} \sigma_o &= 0.8197, & \sigma_u &= 13.6, \\ 2\alpha &= 4.984 \text{ mgrms.}, & (\alpha_{ou} &= 40.71 \text{ mgrms.}). \end{aligned}$					
0.6472	7.473	-8.026	-66.65	-35.81	$28^\circ 36'$

It is seen that these numbers agree well with the determinations obtained from observation of flat drops of mercury (Table X. § 10). But the angle ω_{ou} in the experiments by Gay-Lussac for mercury and alcohol is not to be put $= 180^\circ$.

[To be continued.]

LI. On the Relations of Magnetism and Static Electricity.

By CHARLES W. VINCENT‡.

IT was not the writer's intention to contribute to the Philosophical Magazine on the above subject in the present Number; but an error of rather a serious character in his last communication compels him to add a few lines to it.

At page 300 and at page 302, he describes a pointed iron wire and a pointed magnetic needle as becoming inductively charged, at the point with positive, at the thick end with negative electricity; that (as he need hardly tell electricians) is not so. The

* Laplace, *Méc. Céleste*, vol. iv. p. 496. Poisson, *Nouv. Théor.* p. 147.

† Poisson, *Nouv. Théor.* p. 113. Laplace, *Méc. Céleste*, vol. iv. p. 524.

‡ Communicated by the Author.

idea the writer should have conveyed was that the pointed wire and needle *behaved* as if they had polarity at their opposite ends as regarded the electrifying body, and as if the polarity were so distributed. During the time a pointed needle is being charged the head is attracted, the point repelled from the electrifier; but so soon as the tension of the electricity of the wire or needle approaches that of the electrifying body the thick end is likewise repelled.

The following experiment, which is the converse of the one narrated, page 302, affords an additional illustration of this kind of polarity; at least that is the construction which the writer puts on the phenomena presented.

The magnet was arranged precisely as in the previous experiment; but the test-needle was brought over the other pole, so that the head instead of the point of the needle was the attracted end. Under these circumstances, when the system was charged, the needle endeavoured to get still closer to the magnet, the free end being at the same time thrown more from the pole to which it was before inclining. The needle was then placed at a greater distance from the magnet: on charging the apparatus, the attraction was very visible, the head being drawn towards the magnet from a distance of an inch, and being so held whilst the arrangement continued to be electrified. So soon, however, as the positive and negative sides of the condenser were united, the needle took up its former position in relation to the magnet.

Touching the needle with a piece of copper wire during the process of charging, or after the condensing arrangement was charged, perceptibly increased the existing attraction of the needle for the electrified magnet.

The foregoing experiments were repeated with the horseshoe-magnet as the negative side of the condensing system, the tinfoil-covered glass being superposed as the positive side, the whole being so arranged that the charged tinfoil should be between the poles of the magnet and the test-needle.

Matters being thus arranged, on charging the apparatus as in the previous experiments, the results afforded were in every way precisely the same as when the magnet was the positively electrified body in the system and had no dielectric between it and the needles other than the air.

Both repulsion and attraction of magnetized needles, under the circumstances above described, are accompanied by a marked divergence from the lines of magnetic force in which they would have set if unbiased by static electricity. The distance of one end of a magnetic needle from a magnet being made the same under the influence of the magnetic force and the combined forces, in the latter case the needle tends to put itself in a ver-

tical position as regards that line of magnetic force which, if left to itself, it would accord with.

The writer desires to express a hope that, in those respects in which his opinions may appear heterodox to commonly received theories, it may be remembered that he puts them forth only as forming a possible basis on which his experiments might be built up. The conclusions at which the investigator may ultimately arrive may be diametrically opposed to those on which his present assumptions rest. Priestley, whose opinion all will recognize as of some value on such matters, gives great encouragement to those who do not fear to find their hypotheses fail and their conclusions reversed if by their boldness (it may be audacity) they have nevertheless forwarded the knowledge of the truth.

He says, "It is by no means necessary to have just views and a true hypothesis, *à priori*, in order to make real discoveries. Very lame and imperfect theories are sufficient to suggest useful experiments, which serve to correct those theories and give birth to others more perfect. These, then, occasion further experiments, which bring us still nearer to the truth; and in this method of *approximation* we must be content to proceed, and we ought to think ourselves happy if in this slow method we make any real progress"*.

24 St. Peter's Street, E.

LII. *Note on Calorimetric Researches.* By R. BUNSEN.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

April 20, 1871.

I HAVE just received the enclosed letter from Professor Bunsen. No man guards more jealously scientific honour than he; and the following, whilst it explains why the name of Herschel was not credited with the idea of measuring heat by the increase of volume of melting ice, points out the essential differences between the method proposed by Herschel and that carried out by the illustrious chemist of Heidelberg.

Yours &c.,

HENRY E. ROSCOE.

In a recent Number of Poggendorff's *Annalen* Professor Andrews remarks that more than twenty years ago Sir John Herschel proposed an improvement upon Lavoisier and Laplace's ice-calorimeter, which, according to Andrews, is in principle and even in several details the same as that which I have employed in my new calorimetric researches (*Phil. Mag. S. 4. vol. xli. p. 161*).

* Priestley, 'On Light,' sec. 4, p. 181.

The reason that I was not aware of Herschel's investigation, and therefore did not mention it in my memoir, will probably be understood when I state that it appeared in an appendix to an astronomical work published in 1847, which not only escaped my attention, but even appears not to have come into the hands of physicists themselves; at any rate, as far as I can learn, no one who has been engaged on calorimetric investigations during the quarter of a century which has elapsed since the publication of Herschel's volume has ever mentioned the proposal of the illustrious astronomer, much less either tried or used the method; otherwise I certainly should not have omitted most distinctly to state that the idea of measuring the amount of melted ice by the diminution of volume which thereby occurs is due to Herschel. On the other hand, I may be allowed to take advantage of my friend Andrews's remark to point out, perhaps more precisely than I have done in my memoir, one at least of the most important respects in which the instrument proposed by Herschel differs from that which I have described.

Herschel absorbs the heat which is to be measured by the alteration of volume produced by the ice melting, in a vessel filled with water and floating ice. In my arrangement the same purpose is effected in a vessel made out of a solid mass of artificially congealed ice. The medium employed by Herschel, under the circumstances of the experiment, allows heat to pass through it; that which I use, on the contrary, cannot transmit any of the heat. In regard to the quantity of heat to be measured, my instrument bears to that of Herschel the same relation as a water-tight vessel does to a sieve as regards the measurement of a volume of liquid. We should, however, hardly call two methods the same, one of which measured a liquid with a water-tight vessel and the other with a sieve. The loss of heat occurring in a measurement made with a vessel filled with ice and water is great enough to throw doubt upon any exact calorimetric determination, as may be seen by reference to experiments detailed in my memoir, and as every one knows who has tried to determine the freezing-point of thermometers in a mixture of water and ice instead of in snow. The warmed water which fills the spaces between the pieces of ice in Herschel's apparatus, and which, according to its temperature, either rises or sinks, comes in a few seconds, and without having completely equalized its difference of temperature, into contact with the surface of the glass vessel surrounding the medium, and gives up this heat, which is thus withdrawn from measurement.

The extent of uncertainty which is introduced by such a mode of measurement may be estimated when we remember that, according to experiments given in my paper, more than 30 minutes

is required in order completely to reduce the temperature of half a gramme of water from 100° to 0° . This source of error, which has been the chief cause of the dismissal of Laplace's calorimeter from among the ranks of exact physical instruments, and which is in no way diminished in Herschel's improved form, is entirely removed by the principle which I adopt. According to my method, the bodies whose loss of heat is to be measured by the contraction of the melting of the ice are contained in a vessel perfectly impenetrable for heat; whilst at the same time any error from transference of heat to the air is avoided, as the bodies are placed in a mass of water at 0° C., so that the warmed water, the temperature of which never rises above 4° C., always flows to the lowest part of the vessel.

Heidelberg, April 14, 1871.

LIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 318.]

Feb. 2, 1871.—General Sir Edward Sabine, K.C.B., President, in the Chair.

THE following communication was read:—

“On the Uniform Flow of a Liquid.” By Henry Moseley, M.A., D.C.L., Canon of Bristol, F.R.S.

The resistance of every molecule of a liquid at rest which a solid (by moving through it) disturbs, contributes its share to the resistance which the solid experiences; so that the inertia of each molecule so disturbed and its shear must be taken into account in the aggregate which represents the resistance the liquid offers to the motion of the solid. The motions communicated to the molecules of a liquid by a solid passing through it, and the resistances opposed to them, however, are so various, and so difficult to be represented mathematically, that in the present state of our knowledge of hydrodynamics the problem of the resistance of a liquid at rest to a solid in motion is perhaps to be considered insoluble. As it regards the opposite problem of the resistance of a solid at rest to a liquid in motion (as in the case of a liquid conveyed through a pipe), there are in like manner to be taken into account the disturbances created by that resistance in what would otherwise have been the motion of each individual molecule of the liquid so disturbed.

This problem, however, is by no means so difficult as the other. There is, indeed, a case in which it admits of solution. It is that of a liquid flowing from a reservoir, in which its surface is kept always at the same level, through a circular pipe which is perfectly straight, and of the same diameter throughout, and of a uniform smoothness or roughness of internal surface, and always full of the liquid. The liquid would obviously in such a pipe arrange itself in infinitely thin cylindrical films coaxial with the pipe, all the molecules in the same film moving with the same velocity, but the molecules

of different films with velocities varying from the axis of the pipe to its internal surface. The direction of the motions of the molecules of such a liquid being known, and all in the same film moving with the same velocity, which velocity is a function of the radius of the film, and the law of the resistance of each film to the slipping over it of the contiguous film being assumed to be known, as also the head of water, it is possible to express mathematically

(1st) the work done per unit of time by the force which gives motion to the liquid, and

(2nd) the work per unit of time of the several resistances to which the liquid in moving through the pipe is subjected, and

(3rd) the work accumulated per unit of time in the liquid which escapes—and thus to constitute an equation in which the dependent variables are the radius of any given film, and the velocity of that film. This equation being differentiated and the variables separated, and the resulting differential equation being integrated, there is obtained the formula

$$v = v_0 e^{-\frac{250 r}{l}},$$

where v is the velocity of the film whose radius is r , and v_0 that of the central filament, and l the length of the pipe—the unit of length being one metre, and of time one second.

The method by which the author has arrived at this formula is substantially the same as that which he before used in a paper read before the Society on the “Mechanical Impossibility of the Descent of Glaciers by their weight only,” and which he believes to be a method new to mechanical science. It was indeed to verify it in its application to liquids that he undertook the investigations which he now submits to the Society, which, however, he has pursued beyond their original object.

The recent experiments of MM. Darcy and Bazin* have supplied him with the means of this verification. These experiments, made with admirable skill and precision, on pipes upwards of 100 metres in length, and varying in diameter from 0^m.0122 to 0^m.5, under heads of water varying in height from 0^m.027 to 30^m.714, include (together with numerous experiments on the quantity of water which flows per second from such pipes under different conditions) experiments on the velocities of the films of water at different distances from the axes of the pipes, made by means of an improved form and adaptation of the well-known tube of Pitot. These last-mentioned experiments afford the means of verifying the above-mentioned formulæ. With a view to this verification, the author has compared the formula with sixty of the experiments of M. Darcy, and stated the results in the first two Tables of his paper.

The discharge per 1" from a pipe of a given radius may be calculated from the above formula in terms of the velocity of the central filament. This calculation the author has made, and compared it with the results of eleven of M. Darcy's experiments.

* Recherches Expérimentales relatives au mouvement de l'Eau dans les Tuyaux, par H. Darcy: Paris, 1857. Recherches Hydrauliques, par MM. Darcy et Bazin: Paris, 1865.

Where in the formula which thus represents the discharge from a pipe of given radius, in terms of the velocity of the central filament, the radius is made infinite, an expression is obtained for the volume of liquid of a cylindrical form, but of infinite dimensions (laterally), which would be put in motion by a *single filament* of liquid which traversed its axis; and, conversely, it gives the volume of such a liquid in motion which would be held back by a filament of liquid kept at rest along its axis. Thus it explains the well-known retarding effect of filaments of grass and roots in retarding the velocities of streams.

It is the relation of the velocity of any film to that of the central filament which the author establishes in the above formula. To the complete solution of the problem it is necessary that he should further determine the actual velocity v_0 of the central filament. This is the object of the second part of his paper. This velocity being known, the actual discharge per 1" is known. The following is the formula finally arrived at :—

$$Q = C \left[\epsilon^{-\frac{250 R}{l}} - \frac{250 R}{l} - 1 \right] R^{\frac{1}{3}} h^{\frac{1}{2}} l^{\frac{5}{3}},$$

where

Q = discharge per 1" in cubic metres.

R = radius of pipe in metres.

l = length of ditto.

h = head of water.

C = a constant dependent on the state of the internal surface of the pipe.

The values of this constant C , as deduced from the experiments of M. Darcy are given,

1st, for new cast-iron pipes;

2nd, for the same covered with deposit;

3rd, for the above *cleaned*;

4th, for iron pipes coated internally with bitumen;

5th, for new leaden pipes;

6th, for glass pipes.

The author compares this formula with sixty-two of M. Darcy's experiments, and records the results of this comparison in the last three Tables of his paper.

The paper concludes with an investigation of the rise in the temperature of a liquid flowing through a pipe caused by the resistances which its coaxial films oppose to their motions on one another (or, as it is termed, their *frictions* on one another) and on the internal surface of the pipe. The pipe is in this investigation supposed to be of a perfectly non-conducting substance.

February 9.—General Sir Edward Sabine, K.C.B., President, in the Chair.

The following communication was read :—

"Observations of the Eclipse at Oxford, December 22, 1870." By John Phillips, M.A., D.C.L., F.R.S.

At my observatory, situated about one third of a mile eastward from the great establishment founded in the name of Dr. Radcliffe, the

beginning of the eclipse was obscured by a passing cloud: the end was recorded at $13^h 38' 38'' = 1^h 35' 0'' \cdot 9$ Oxford mean time.

The progress of the obscuration was observed at unclouded intervals in the first half of the period, continuously during a clear sky in the latter half. Finding it impracticable to observe and measure with ordinary micrometers in the early part of the phenomenon, I arranged to throw the image on a screen, and make my measures on it.

The driving-clock was affected by the extreme cold, so as to make it difficult to keep the sun's image to one place, and it was convenient for other reasons sometimes to shift the image vertically; the method which I employed, however, was independent of these displacements, and allowed of as many measurements of the cusps as might be desired.

It consisted simply in marking at any moment with pencil the situation of the cusps on the screen, and appending to each dot the time by the sidereal clock. Joining, after the eclipse, these dots by a straight line, and then transferring a parallel line of equal length to meet internally a circle representing the limb of the sun, of the same diameter as the solar image, the chord of the cusps at the given time was obtained, from which, by an easy method, the place of the moon's centre at the moment was derived. The apparent diameters of the sun and moon were obtained by measure of arcs on the screen.

The diagrams exhibit the whole process. In diagram fig. 1, four of the lines are drawn from the dots on the screen, AA, BB, CC, DD.

In fig. 2, equal and parallel lines are transferred to the solar circle, whose centre is S, so as to touch it internally at A' A', B' B', C' C', D' D'. For each of these lines the centre of the moon's place is marked (A'', B'', C'', D''); thus the line of the motion of the moon's centre is given, and the phase of greatest obscuration determined.

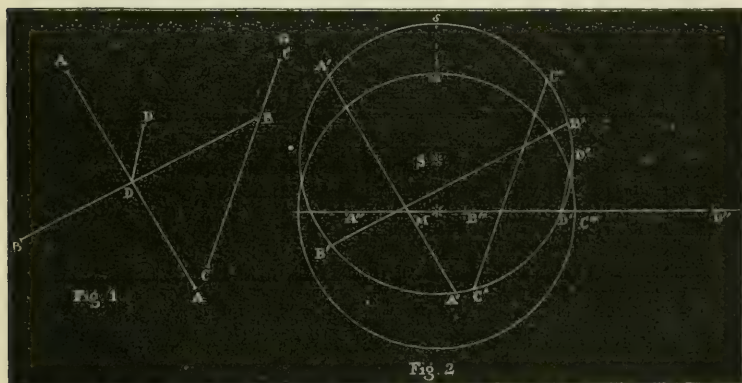


Fig. 1. AA, BB, CC, DD, are lines joining the dots marking the cusps at four successive epochs during the eclipse.

2. A' A', B' B', C' C', D' D', are four lines equal and parallel to AA, BB, CC, DD in fig. 1, and made to touch internally the solar circle, whose centre is S; *ms* the sagitta at the moment of greatest obscuration. The moon's path passes below A'', above B'', and nearly coincides with C'' and D'', which are the places of the moon's centre for the cusps A', B', C', D'.

The line of motion of the moon's centre is obtained by ruling through the mid points between A'' and B'', B'' and C'', C'' and D''. The point on this line reached by the moon's centre at the moment of greatest obscuration is found by bisection in M. Drawing through M and S the bisecting line of greatest obscuration, the length of the sagitta *ms* is determined.

It is found by these observations that,

The sun's diameter being taken at	530
That of the moon is	540
The length of the sagitta <i>ms</i> is	100

These numbers, according to the proportions given in the Nautical Almanac for the Radcliffe Observatory, would have been:—

Diameter of the sun	530·0
Diameter of the moon	538·8
Length of sagitta	99·1

The agreement is quite close enough to justify the belief that, in skilful hands, the method described may be in some cases very useful, it being by no means limited to eclipses. It is so simple that one can hardly suppose it not to have been already employed; but I have met with no notice of such being the case.

During the progress of the eclipse three thermometers were observed. One north of the house, screened from the sun and sky, sank from 26° at $11^{\text{h}} 40^{\text{m}}$ to $24^{\circ} 4'$ at $1^{\text{h}} 25^{\text{m}}$. One south of the house, indirectly influenced by solar radiation on neighbouring objects, rose from $26^{\circ} 75'$ at 11^{h} to $27^{\circ} 8'$ at noon, then sank to 26° at $12^{\text{h}} 40^{\text{m}}$, and rose to $27^{\circ} 3'$ at $1^{\text{h}} 35^{\text{m}}$. A third, on grass open to the sky, sank from $27^{\circ} 8'$ at $11^{\text{h}} 40^{\text{m}}$ to $23^{\circ} 5'$ at $1^{\text{h}} 25^{\text{m}}$, and remained at this point till $1^{\text{h}} 35^{\text{m}}$. Though on a limited scale, the influence both of solar and sky radiation is traceable in these observations.

ROYAL INSTITUTION OF GREAT BRITAIN.

Friday, February 3, 1871.—“On some Experiments on Successive Polarization of Light made by Sir C. Wheatstone.” By W. Spottiswoode, Esq., Treas. R.S. and R.I.

The experiments which formed the subject of this discourse were made by Sir Charles Wheatstone some years ago, but the pressure of other avocations delayed their publication. The term “Successive polarization” was applied by Biot to denote the effects produced when a ray of polarized light is transmitted through a plate of rock crystal cut perpendicularly to the axis. The plane of polarization is found to be changed on emergence and through a different angle for each homogeneous ray. The introduction of instrumental means for converting the plane polarization of the ordinary apparatus into successive, or, as it is more commonly called, circular polarization, and the explanation of the phenomena thence arising, constitute the main purpose of the communication.

Polarized light is distinguished from common light by the presence of certain peculiarities not ordinarily found; but the peculiarities in question cannot be discerned by the unassisted eye, and require special instrumental appliances for detection. A simple mode

of bringing light into the condition in question is by allowing it to pass through a plate of the crystal called tourmalin, cut parallel to the axis; and if the light be then examined by causing it to pass through a second similar plate, held parallel to the former and caused to revolve like a wheel in its own plane, it will be found that the intensity alternately diminishes and revives, being zero for two positions 180° from one another, and a maximum for two positions at 90° from each of the former. This combination of tourmalins constitutes in fact a polariscope, which in general consists of two parts, counterparts of each other—the first for bringing common light into the condition in question (in other words, for *polarizing*), the second for examining or *analyzing* the light.

The explanation of this fundamental phenomenon is as follows:—The vibrations upon which the sensation of light depends, may in ordinary light take place in any direction in a plane perpendicular to the ray. By the process of polarization they are all brought into one direction, still, however, perpendicular to the ray; so that throughout the entire ray they lie in one plane. On this account the polarization here considered is called plane polarization. There are other kinds of polarization, such as circular and elliptic, whose names are derived from the curves, or orbits, described by the vibrating particles.

There are also other methods for producing plane polarization beside that above described (*e. g.* reflection at particular angles from the surfaces of transparent media, transmission through parallel plates of glass, &c.); but as they all agree in reducing common light to the same condition, it is unnecessary for the present purpose to allude to them more in detail.

If a ray of polarized light fall upon a plate of doubly-refracting crystal, it is divided into two, whose vibrations lie in planes perpendicular to one another. These rays traverse the crystal with different velocities, and therefore emerge with a difference of phase. On entering the analyzer, the vibrations of both rays are resolved into one plane. If the plane of vibration of the analyzer be parallel to one of those of the crystal, one ray will be cut off, the other will be transmitted without change. In any other position of the analyzer the transmitted portions of the two rays will interfere so as to produce colour; and if the analyzer be then turned through 90° , the portion of the original light cut off in the first position will be transmitted, and *vice versa*.

Of this theory the following are the experimental results:—If a plate of doubly-refracting crystal, *e. g.* selenite, be placed between the polarizer and analyzer, and turned round in its own plane, it will be found that in certain positions at right angles to one another no effect is produced. These may be called neutral positions. In all other positions the field is tinted with colour, which is most brilliant when the plate has been turned through 45° from a neutral position. If the analyzer be turned, the crystal remaining still, the colour will fade, and entirely vanish when the angle of turning amounts to 45° . From this position the complementary colour will

begin to appear, and will be brightest when the angle of turning amounts to 90° . The colour depends upon the thickness of the crystal ; so that by a suitable preparation any arrangement of colours may be produced.

So far for plane polarization. The principal of circular or successive polarization, as regards the present purpose, is as follows :—

If two sets of rectilinear vibrations lying in planes perpendicular to one another meet and combine, the resulting vibration will be curvilinear, whose form and position depends upon the difference of phase of the components. If the second set be in advance or in rear of the first by a quarter of a wave-length, the resulting vibration will be circular ; but the motion will in one case be direct (like the hands of a watch), in the other it will be reverse.

If two sets of circular vibrations in opposite directions meet and combine, the resulting vibrations will be rectilinear, and the position of their plane will depend upon the difference of phase of the components. If the second set advance upon the first, the plane of resultant vibration will undergo direct rotation ; if it recede, it will undergo reverse rotation.

If in such an experiment white light be used, the vibrations of the different component prismatic colours will (on account of their different wave-lengths) undergo different retardation ; and consequently the resultant vibrations will lie in different planes, arranged in prismatic order. The order will be from red to violet, or *vice versa*, in accordance with the law stated above.

If a ray of plane-polarized light fall upon a metallic reflector, it is divided into two, whose vibrations are respectively parallel and perpendicular to the reflector ; and the latter is retarded behind the former by a difference of phase depending upon the angle of incidence. If the plane of vibration of the incident ray be inclined at an angle of 45° to the plane of incidence, the two rays into which it is divided have nearly the same intensity. At an angle, nearly 45° , which varies with the metal employed, but which is perfectly definite, the intensities become accurately equal. And, further, if the angle of incidence have a particular value, dependent upon the nature of the metal (for silver 72°), the retardation will amount to a quarter of a wave-length. These two rays, on leaving the reflector, will recombine, and, in accordance with the laws above given, will in the last-mentioned circumstances become a circularly polarized ray. Lastly, the direction of motion in this circular ray will depend upon the side on which the original plane of vibration is inclined to the plane of incidence ; if, when it is inclined on one side, the circular ray becomes right-handed, then, when it is inclined on the other, it becomes left-handed.

Reverting then to the phenomena of double refraction, produced by a plate of crystal cut parallel to the axis on a plane-polarized ray, let the crystal be placed in such a position that the planes of vibration of the two resulting rays are inclined at angles of 45° on the two sides respectively of the plane of incidence ; and let there be interposed between the crystal and the analyzer a silver plate at an

angle of 72° to the direction of these emergent rays. Each of these rays will, in virtue of the principles enunciated above, be converted by reflection into a circularly polarized ray; but one will be a right-handed, and the other a left-handed ray; and the difference of phase produced by the doubly-refracting plate will be undisturbed by the reflection. This difference of phase depends, as is well known, upon the wave-length—in other words, upon the colour of the light; so that the two circular rays will combine to form a plane-polarized ray, whose plane of vibration depends upon the difference of phase, *i. e.* upon its colour. And if, finally, the light be then examined by an analyzer in the usual manner, we shall have all the phenomena of circular or successive polarization.

From what has been stated above, it appears that the direction of motion in the two circular rays, and consequently the order of colours produced, depends upon the position (to the right or left of the plane of incidence) of the ray which has been most retarded in its passage through the crystal plate. If, therefore, the plate being in a given position, the colours appear in an ascending order, then on turning the plate through 90° in its own plane, or on turning it over about an axis in the plane of incidence, the swifter and the slower rays will change position, and the order of colours will be reversed.

The reversal of the order of colour may be exhibited in another way. Uniaxial crystals are divided into two classes:—one, called positive (*e. g.* quartz), in which the extraordinary ray moves more slowly than the ordinary; the other, called negative (*e. g.* Iceland spar), in which the ordinary ray is the slowest. If, therefore, a plate of quartz placed with its axis at 45° on one side of the plane of incidence give the colours in one order, a similar plate of Iceland spar similarly placed will give them in the reverse order.

The same principles apply to the case of biaxial crystals cut parallel to a plane containing the two optic axes. A ray of plane-polarized light transmitted through such a plate is divided into two, whose vibrations respectively bisect the angles formed by the two axes; the line which bisects the smallest angle is called the intermediate section, and the line perpendicular to it the supplementary section; and the order of the colours depends upon the relative velocity of the two rays. In selenite the ray whose vibrations lie in the supplementary section is the slowest; in mica it is the swiftest. Hence these two crystals will, all other circumstances being alike, give opposite orders of colour, and may be regarded as positive and negative respectively, like quartz and Iceland spar.

The phenomena by which these principles may be illustrated are very numerous and varied, but are better seen than described.

LIV. *Intelligence and Miscellaneous Articles.*

ON A BAROMETER WITHOUT MERCURY. BY A. HELLER, PROFESSOR OF PHYSICS IN THE OBER-REALSCHULE IN OFEN.

THE atmospheric pressure at a given time and place may be determined either by means of an apparatus which depends upon the principle of communicating-tubes, or by determining the specific

gravity of the air, and calculating therefrom the expansion at the time. The former method is in universal application in the mercury barometer; the latter one has not hitherto been used in barometric measurements.

The apparatus which I have prepared and employed, and by which the atmospheric pressure can at all times be determined, consists of a scale-beam, to the ends of which are screwed two bodies nearly equal in weight but greatly differing in volume,—a hollow sphere and a solid cylinder*. On one end of the beam is a mirror which is approximately at right angles to the axis of the beam. At some distance from the apparatus is a telescope with a vertical scale, the image of which in the mirror is observed by means of the telescope. It is clear that when there is any change in the expansion of the air in the vicinity of the apparatus, the beam will indicate varying angles with the horizon, which angles may easily be read off in the mirror by means of the telescope.

The variations of the scale-beam in consequence of alterations in the pressure will not amount to much, if the dimensions of the apparatus are moderate; but the use of Poggendorff and Gauss's method of reading affords such a degree of accuracy that, as a brief calculation shows, under assumptions which are easily realized, the changes in the position of the beam can be measured with far greater certainty and accuracy than the height of the mercurial column in the ordinary barometer, provided the whole construction is light, and that its centre of gravity is at a short distance from the knife-edge of the beam.

We will assume that the beam in dry air at the temperature 0° and under a pressure of 760 millims. is in equilibrium, and that we have:—

Q , the true weight (reduced to vacuum) of the hollow sphere;

Q_1 , that of the solid cylinder;

V_0 and v_0 , the corresponding volumes at 0° ;

ϕ and ϕ_1 , the corresponding coefficients of expansion;

α , the coefficient of expansion of air;

B , the expansion of air; t , the temperature;

T , the expansion of the aqueous vapour in the air;

G , the weight of the entire construction;

h , the distance of its specific gravity from the axis of rotation;

u , the length of the arm of the lever.

The tangent of the angle of deflection of the beam is then

$$\log \omega = \frac{Q - Q_1 - V_0(1 + \phi t) - v_0(1 + \phi_1 t)}{1 + \alpha t} \cdot 0.001293 \cdot \frac{B - \frac{3}{8}T}{760 \text{ millims.}},$$

$$G \frac{h}{a}$$

* Thus far the apparatus I have employed is in principle identical with Guericke's dasymeter or manometer. That arrangement is, in fact, a balance in which equilibrium is established by means of weights. Gerstner (senior), who has constructed the most accurate manometer, used it in determining the specific gravity of air under varying conditions of pressure and temperature. This he effected by adjusting a sliding weight on a graduated scale-beam. The apparatus fell into disuse in consequence of the subsequent more accurate measurements.

whence

$$\frac{d\omega}{dB} = -\cos^2 \alpha \frac{V_0(1+\phi t) - v_0(1+\phi_1 t)}{G \frac{h}{a}} \frac{1}{1+\alpha t} \frac{0.001293}{760}$$

gives us the measure of the delicacy of the instrument, which therefore reaches the maximum about the horizontal position of the beam. As, moreover, the greatest deflection only amounts to a few degrees, the delicacy can be regarded as constant. On a certain assumption, calculation gives for an increase in the pressure of 1 millim. a deflection of a few millimetres (4 to 5) of the scale.

But the present apparatus not merely gives a determination of the pressure of the atmosphere with at least as great a degree of accuracy as the mercury barometer, but appears also to have several material advantages over this, as it is liable to far smaller errors.

In every instrument which serves for the measurement of a variable force it is essential that it require the movement of as small masses as possible, and the more so the quicker the changes which it is to indicate; for under certain circumstances results greatly varying from actual fact would be obtained.

In the mercury barometer the movement of a column of liquid of considerable weight is essential in indicating changes in the atmospheric pressure; this heavy column of mercury will only be set in motion if the impulses are accumulated in such a manner that they can overcome the resistance along the entire surface of the tube and acquire a certain velocity. If the change in the pressure take place with great velocity, the mercury barometer, in virtue of its inertia, will never indicate the actual pressure, and never its highest or lowest value.

From its small size, as well as from the small resistance to its motion, the above apparatus is far less exposed to this objection; and we shall probably be enabled by its means to follow such rapid changes in the pressure of the atmosphere as in the ordinary barometer can only be expressed by mean values.

A further disadvantage of the ordinary barometer is that, as a matter of fact, the Torricellian vacuum always contains mercury vapour of small tension and also atmospheric air, and the more of this the longer the instrument has been in use. And although a correction may be introduced for the vapour of mercury, its action upon the capillary depression, as well as the air in the Torricellian vacuum, cannot be accurately allowed for.

Finally, the use of the mercury barometer presupposes filling it with mercury of a precisely definite specific gravity, which also presumes an impossible condition; for chemically pure mercury cannot be obtained without difficulty, and does not keep, but partially oxidizes after a time. Now experiments have shown that the smallest impurity in the barometric liquid has a material influence on the capillary depression.

Defects analogous to these the new instrument does not seem to have, provided the beam be constructed with that care which is usually bestowed upon delicate balances.

The author intends in due course to report upon comparative measurements with mercury barometers and this instrument.—Poggendorff's *Annalen*, February 1871.

Ofen, December 28, 1870.

INVERSION OF THE SODIUM-LINE. BY A. WEINHOLD.

The usual mode of inverting the sodium-line (in which feebly luminous sodium vapour is interposed between an ignited body and the slit of the spectroscope) requires great brightness in the white light, in order that the quantity of light absorbed by the sodium vapour may be considerably greater than that radiated by it, and thus the sodium-line be considerably feebler than the adjacent parts of the spectrum.

It was to be expected that the inversion would take place more readily if the brightness of the parts of the spectrum adjacent to the sodium-line could be as much increased as the brightness of the sodium-line is increased by the luminosity of the sodium vapour.

This can be very easily effected. A small petroleum lamp is placed in front of the slit of a small spectroscope consisting merely of a tube (without lens) and a powerfully dispersing prism; and a spirit-flame *intensely* coloured by common salt is so placed between the spectrum and the eye that it covers the entire spectrum; there at once appears a very distinct dark sodium-line, while the same spirit-flame placed between the petroleum lamp and the slit brings out the sodium-line bright and luminous.

In the first position the entire spectrum seems illuminated by the yellow light indicated by it, and therefore the sodium-line is less bright than the adjacent parts of the spectrum by the entire amount of the absorption; while in the second position these parts are not at all altered or the sodium-line enfeebled by the absorption of sodium vapour, but strengthened by its own radiation.

If the lamp-wick be only rubbed with salt, it is only in the first few seconds after lighting that it is intense enough to produce a distinct inversion; it is better before filling the lamp with alcohol to dilute this with water and then saturate it with salt; occasional rubbing of the wick between the finger is advantageous even here, to make the sodium-line of a dense black.

There is a slight difficulty which very short-sighted people experience in finding the right accommodation for the slit, and not for the alcohol-flame. This difficulty is lessened by placing a wire in front of this slit, which then appears as a dark horizontal line in a continuous spectrum, and shows how the proper accommodation is to be effected.

The use of a spectroscope with telescopes gave no good results. Owing to the small distance of the eye, the flame cannot be placed between the eyepiece and the eye; and placed between the prism and the object-glass it confuses the picture too much, owing to the currents of hot air. From the construction of my apparatus an adjustment in an incision of the slit-tube close behind the slit, or between the object-glass and eyepiece of my telescope near the collecting-lens, was not possible.—Poggendorff's *Annalen*, Feb. 1871.

THE
LONDON, EDINBURGH, AND DUBLIN
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

[FOURTH SERIES.]

JUNE 1871.

LV. *On Approach caused by Vibration. By various Authors.*
Edited by FREDERICK GUTHRIE.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

THE following communication is divided into three parts, as follows:—

PART I. *Synthetical View of the Forms and Forces of Matter.*
By DR. JULES GUYOT.

PART II. *Acoustic Repulsion and Attraction.* By K. H. SCHELLBACH.

PART III. *Approach caused by Vibration, being five Letters addressed to me by* SIR WILLIAM THOMSON.

Dr. Guyot's paper was first brought to my notice on reading Herr Shellbach's second paper. On turning to the original I found that Dr. Guyot had anticipated so many of Herr Shellbach's and my own experimental results, that it became an obvious duty on my part to bring Dr. Guyot's claim to be the experimental discoverer of this highly interesting class of phenomena before your readers, to most of whom Dr. Guyot's experiments may be as unfamiliar as they were a short time ago to myself.

Your readers will also certainly be interested in Herr Shellbach's experiments on acoustic repulsion and attraction. It appears from Herr Shellbach's paper No. 1 that his experiments therein described were performed independently of, and nearly at the same time as mine. Herr Schellbach's papers 2 and 3 are specially interesting, as they bear upon the conclusions drawn by Sir W. Thomson concerning the same subject.

Phil. Mag. S. 4. Vol. 41. No. 275. June 1871. 2 E

Some of these conclusions are contained in five letters which Sir W. Thomson has done me the honour of addressing to me, and which I give with permission on pp. 423-429, in the belief that they are of too great interest and importance to remain unpublished. The first of these letters formed the substance of a communication to the Royal Society of London, and appears in the Proceedings of the Royal Society for 1871, No. 125, p. 271.

I need not point out that great additional interest has been given to the subject by Professor Challis's interesting paper in the April Number of your Magazine.

I remain, Gentlemen,

Your obedient Servant,

FREDERICK GUTHRIE.

I. *Synthetical View of the Forms and Forces of Matter.*

By Dr. GUYOT*.

All who occupy themselves with general physics have doubtless read Mr. Grove's 'Correlation of the Physical Forces,' either in the original or in the French translation of M. l'Abbé Moigno, published in Paris in 1856 with notes by M. Séguin, sen. Mr. Grove published the first edition of his work in 1848, and he himself dates its commencement from 1842.

M. Séguin, sen., while admiring Mr. Grove's synthetical train of thought, claims, so to say, the priority thereof for his uncle, the celebrated Mongolfier, or rather for himself as his uncle's intellectual legatee. Nevertheless he declares that it was only in 1848, after Mr. Grove's publication, that he thought of disinterring from its crypt the scientific treasure of which he was heir. Soon afterwards (in 1852) he founded the *Cosmos* under the editorship of the learned Abbé Moigno, and published a great number of articles, which, together with the notes added by him to the translation of Mr. Grove's work, complete a synthetical theory which has only, as a real and certain date, the date of its publication.

M. Love, a most distinguished engineer and, I imagine, one of our colleagues, has just published this year an extremely interesting work in the same direction, or which is at least inspired by the same synthetical idea of the correlation of the physical forces. This work is entitled *Essai sur l'Identité des agents qui produisent le Son, la Chaleur, la Lumière, et l'Électricité* (Paris, 1861). Mr. Grove still remains at present the founder of the school, the first popularizer of the idea.

He would indeed remain so did not the dead arise from their

* "Coup d'œil synthétique sur les formes et les forces de la matière. Note sommaire présentée au Cercle de la Presse Scientifique dans la Séance du 4 Juillet 1861" (*Presse Scientifique*, 1861, t. iii. p. 130).

tombs. Unfortunately I am compelled to resuscitate them. The school is broken up and the disciples are scattered when M. Love admits an electric fluid, when M. Séguin reestablishes a luminous fluid, and I raise before the readers of the *Presse Scientifique* the obituary stone in honour of my 'Elements of General Physics,' published in 1832, ten years before Mr. Grove, twenty years before M. Séguin, thirty years before M. Love.

God forbid that I should seek in any way to detract from Mr. Grove's work, from M. Séguin's publications, or from M. Love's book. I recognize their great value, and I leave that intact. They have maintained the analogy and the identity of the physical forces by fact, arguments, and calculations which have strengthened the idea and familiarized it to a great many minds. But they have not entirely deprived it of obscurity, and have not yet given it that character of simplicity and universality demanded by a simple and prolific law of general physics.

It is not my purpose, moreover, to discuss a sterile question of priority; but I think I may be useful to science in proposing myself as a workman in the workshop of scientific progress. Respecting the work of others, ought I not to make known my own, and state since when and by what title I have been apprenticed?

Only in this sense I offer to the readers of the *Presse Scientifique* my 'Elements of General Physics,' and refer their date to 1832. At this epoch I had established and proved:—(1) that there exist two kinds of matter, identical in principle, essentially different in form; (2) that there exist *two kinds* of movements, identical in principle, essentially different in their manifestations—the movement of *translation* or bodily ("*du corps*"), and the movement of *vibration* or molecular; (3) that movement is *essential* to matter and *proportional to mass*; (4) that the movement of translation and the movement of vibration are reciprocally *complementary* and change into one another; (5) that attraction (? gravitation, F. G.), magnetism, and electricity are the most general consequences of molecular motion; (6) that the second degree of molecular motion constitutes heat; (7) that the third degree of molecular motion constitutes light; (8) that taste and smell are relative manifestations of molecular motion; (9) that sound is a mixed motion, compounded of the motion of translation or "bodily," and the motion of vibration or molecular; (10) finally, that all the properties, either physical or physiological manifestations, of bodies are derivatives of the translatory or vibratory motion.

What I advanced at the above date I am now prepared to establish and demonstrate with greater clearness and confidence,

because for thirty years my mind has not ceased to work, to observe, and to reason in the same direction. And I have been thus led to valuable discoveries—discoveries which ought to serve for the mechanical proof of the cause and effect of attraction. As starting-point (*point d'appui*), and as base of the synthesis which I am about to develop, I ought again to call the attention of the readers of the *Presse Scientifique* to my work on the movements of air and the pressure of air in movement (Paris, 1835), a work which experimentally demonstrates the attractions of bodies suspended in space by sonorous bodies in vibration.

The subject which I am about to attack is so vast and difficult to explain, that I cannot commence it without having previously asked for the kind and patient attention of my readers. I shall certainly announce a number of propositions which will appear more than doubtful, and I cannot stop to discuss and prove them one by one; but, by the patience and kindness of my readers, they will become evident as they are developed. I therefore ask for provisional faith; and I think it is in harmony with the spirit of my readers to grant me this preliminary faith in regard to all matters advanced, with the reserved privilege of judiciously deciding after having heard. In fact, knowledge acquired and established, bases its arguments on the principles and facts which it has accepted.

The spirit of discovery is chiefly supported by principles and facts which are unknown or not received.

Knowledge is therefore the born and legitimate adversary of every truth which seeks other bases than its own. It is often observed that learned official bodies energetically and for a long time reject truths and facts which afterwards take first rank amongst those adopted by science.

If I am not mistaken, the readers of the *Presse Scientifique* are constituted so as to hasten all kinds of progress by avoiding the principal and necessary fault of accepted science—namely, that of judging without hearing, or of judging according to ancient laws the novelties which are presented to it, and of pronouncing impossible every thing which is not derived from its codes, and still more so that which oversets them. From this point of view the *Press Scientifique* has a strong *raison d'être*; and its foundation will render incalculable services for ever, because, for ever, present knowledge will repel future knowledge until the latter shall have become achieved science; and then the latter, being received as orthodox science, will in its turn become the enemy of that which it does not know. Such is the continual and legitimate contest between the past and the future, between tradition and the work of progress. The rea-

ders of the *Presse Scientifique*, by undertaking to entertain all new scientific theories without denying and without rejecting them *à priori*, perform a function of the highest importance. Official science is conservative. The readers of the *Presse* place themselves at the head of scientific progress.

On Matter and its Forms.

Before considering matter in the exercise of its forces (that is, of its movements), it is well to make, so to say, an inventory of the principal forms under which it presents itself directly to our senses, or indirectly to our sense of induction.

The form of matter which falls the first under our senses is that of bodies considered isolated. Bodies affect three forms, the solid, liquid, and gaseous. In nature we usually see solids grouped together, liquids united in considerable expanses, and gases forming immense zones. In these collective forms the solids constitute solid *media*, liquids liquid *media*, and gases gaseous *media*. The term medium is properly applicable to solids, liquids, and gases when we consider a body contained in or surrounded by them.

Media grouped in spheroidal masses present us matter in the form of the heavenly bodies—the sun and stars; in the form of planets—Mercury, Venus, the Earth, Mars, the new smaller planets as well as the more ancient, Ceres, Juno, Pallas, Vesta, Jupiter, Saturn, and Uranus; and in the form of the earthly satellites and those of Jupiter, Saturn, and Uranus.

If we descend again to the earth we learn that solids, liquids, and gases are formed of different substances, simple or compound, called mineral. We see there organized parasites generally fixed in the solid medium, and growing in the liquid or gaseous medium: these are plants. Finally, we see other systems of independent bodies which are also parasites of the earth, which climb, walk, swim, or fly upon or in the solid, liquid, or gaseous media: these are animals.

Science, by induction or experimental deduction, takes us further: it shows us all the bodies of the earth formed of infinitely small molecules, simple or compound, remaining always the same, whether they constitute minerals, vegetables, or animals.

Atomic elements are exceedingly few in number: twelve or thirteen metalloids and forty or fifty metals, suffice to give rise to the millions of varied forms of all the bodies which we know on the earth; and chemical philosophy readily leads us to suppose that these elements may be reduced to one or two.

Such is the totality of the forms assumed by pressible, ponderable matter which is atomized (that is, grouped in atoms), a

totality which extends to the whole universe and occupies all space. I say that this matter grouped in atoms *occupies* all the universe; I do not say that it *fills* it, because the universe is *filled* by elementary matter,—simple, subtle, impressible, imponderable, not atomized or grouped in atoms (*non atomisée, non groupée en atomes*). Not but that its element is an atom, for it is indeed the absolute atom; but this atom is free from all combination, it is infinitely more subtle than any atom of coercible matter.

The existence of this incoercible matter has always been admitted by the greatest philosophers and the greatest physicists under the name of the subtle substance, or, more generally, æther.

Æther, in fact, constitutes the fourth state and the fourth medium of matter. It is the most abundant of the states; it is the medium of media. It contains all the stars, all the planets, all the satellites. It fills up all the intervals of their atmospheres, of their oceans, and of their solid cores. It penetrates into all gaseous, liquid, and solid bodies, and fills up the intervals of their molecules. In a word, æther is to some extent, in regard to the ponderable matter of the universe, what “mother-liquors” are to the crystalline matter which they hold in solution, and to the crystals which are formed within them. Æther is the mother-liquor of the world. All space void of coercible matter is occupied by it. Not only is there no such thing as an absolute vacuum in nature, but, on the contrary, ponderable and imponderable matter are in an absolute and relative condition of enormous tension. The equilibrium and the phenomena of the world only exist under the condition of the constant pressure of the incoercible matter upon the coercible matter and the reaction of the coercible matter upon the incoercible.

On Movement, or the Forces of Matter.

Matter has only one property—namely, *movement*. Movement can only exhibit itself to our senses and to our spirit of induction through matter and in matter. And, reciprocally, matter is only perceptible to our senses and comprehensible by our minds by its movements. Movement is inherent in and essential to the smallest atoms, as well as to the greatest material systems. In the whole universe we cannot discover a single particle of matter which is in absolute rest. Matter and movement are two creations of the same principle; they are consubstantial, and accordingly proportional to one another.

The first notion which we can have of motion cannot arise from the infinitely small molecules of bodies, because they escape the impressionability of our senses. They reach us, there-

fore, from the change of place effected under our eyes by bodies themselves. Hence motion has been defined as the motion of a *body* from one point of space to another. In order that this definition might be exact, it would be necessary to say, "motion is the passage of *bodies and atoms* from one point of space to another," because a particle, a molecule which we can neither see nor touch, may also change place in the body of which it forms part, and such is clearly a motion identical in principle with the motion of bodies. The difference of the two motions is only relative; the one is exterior to, the other is interior, in the body. But, wonderful to relate, observation of all the phenomena of nature shows that they transform themselves the one into the other, and are thus mutually connected and complementary. The more of exterior motion a body accomplishes, the less is there of internal motion, and inversely. In other words, the same quantity of matter always possesses the same amount of motion.

The inertia of matter is therefore an error. "What does it signify?" say the learned who are more mechanicians than philosophers; "if the hypothesis of inertia permits us to calculate all the facts of equilibrium of motion and of force which occur on the surface of the earth, or rather in our physical and mechanical operations, that is all we want. Whether an hypothesis be true or false, if it be in accord with facts, if it be a sure guide for practical questions, we call it true."

I grant that a mechanic or practical man may hold such language; but they who feel the importance of truth, they who understand that its quest is the most beautiful mission of the human mind, and that its discovery and enunciation infallibly guide humanity in the path of moral and material progress, such, I say, will never admit that a false hypothesis can have the same value as a true one, they will never allow that the atmospheric vacuum is as well explained by the horror of nature as by the weight of the atmospheric column; and yet these two explanations express equally well the same fact. Inertia is an hypothesis as little refined as that of the horror of a vacuum; it explains nothing, and stereotypes error in face of the most brilliant truths. Does inertia explain the motion of the heavenly bodies? Does it explain the motions of animals? No. But the sidereal systems comprehend all known matter; and animal organizations form the last term of material combinations. Inertia therefore remains impotent in regard to the spontaneous phenomena of the one or the other kind; between the alpha and omega of the world it only claims a puny territory, namely the relative movement and rest of bodies in equilibrium amongst themselves in a system full of life.

Although the *exterior* and *interior* motions of bodies are one and the same thing, yet the manifestations and effects are very different upon our senses and our organization, and not less different in their physical and chemical actions upon inanimate bodies.

To study these two great divisions of motion we shall designate them by their most generally received names, *translation* and *vibration*.

Motion of Translation.

The motion of translation is, as we have said, the passage of a body from one point of space to another. Is this apparent and external motion engendered in the heavenly bodies by the internal motion of their molecules as it is with animals? Or is it rather primitive and spontaneous? and do the molecules of the heavenly bodies only enter into vibration when their movement of translation cannot satisfy the sum total of motion essential to their mass?

This question solves itself after the study of vibration. For the present it is sufficient to say that the movement of translation, be it the cause or the effect of the movement of vibration, always strives to take place in straight lines; and if bodies move in curved or broken lines, their change of direction is due to some dominant influence.

The motion of translation of all bodies tends to take place with the same velocity; and this velocity is immense and indefinite. But bodies do not move with the same force, unless they possess the same number of elementary atoms. Velocity resides in the atom, and the force of motion in the number of atoms; and this stands to reason. Twenty men have only the velocity of one man, but they have the strength of twenty. Nevertheless force only exists and only appears as the resistance to velocity. The smaller the velocity becomes, the greater is the force, and inversely. So that if the velocity were infinite (*absolue*) for each atom of a body, the force would be zero in a body of one as in a body of a thousand atoms. And inversely, if the velocity were completely stopped, the effort of the atom or body would attain its maximum. The forces of nature are therefore only resistance to motion in direction or velocity, or the production of motion with a velocity and direction which overcome certain obstacles. This oscillation between force and velocity is nothing but the ceaseless transformation of motion of translation into motion of vibration, and *vice versa*. And this, I hope, will appear from the study of the motion of vibration, a study the least advanced, but the most necessary for the understanding of general physics.

After the study of vibration we shall have to explain the movement of translation of the heavenly bodies and of planets, and satellites on their axes, the orbital motion of planets and their satellites, the movement of atmospheres and oceans, the motions of sidereal systems, the motions of animals and the motions of mechanical and physical translation found on the surface of the earth.

On the Motion of Vibration, its varieties and their effects.

There is no single material molecule in the universe which does not accomplish a motion of vibration more or less extended and more or less rapid. More rapid if it be less extended, more extended if it be less rapid. The simple atoms of æther, the systematized atoms of media, gaseous, liquid, and solid, those of each of the different bodies (elements) which form the three latter media execute, each in the place it occupies, the movement of vibration proper to it.

Is vibration a motion to and fro, or a motion of oscillation from one molecule to another? Is it a simple orbit, or one accompanied by axial rotation? Is it an elliptic or a spiral motion? Genius will find this out; science will ultimately determine it: for the present it is sufficient to know that all the molecules of bodies, both the densest and the rarest, of platinum as well as of hydrogen, are at a distance from one another, isolated from one another, free from one another, and that this distance, this isolation, this liberty of association can only be due to their permanent activity, to their vibration, greater or less, quicker or slower. Each molecule is a power or force in function, a movement of vibration which influences surrounding molecules in the way of contact or cooperation; and each body is the resultant (*résumé*), the total of these forms of which each molecule is the unit. The more dense and the larger a body is, the greater is the sum of the motions it has to perform. Whatever of these motions it does not accomplish in translation, it accomplishes in vibration, and *vice versâ*.

Vibration includes three components—*mode*, *number*, and *amplitude*. The *minor* mode constitutes electricity, the *mean* mode heat, and the *major* mode light. Number constitutes the degrees of electricity, of heat, and of light. Amplitude constitutes the capacities, the dilatations, and the gaseous, liquid, and solid states. The power of attraction is common to all these modes of vibration.

These three orders of vibratory movement of the molecules of ponderable bodies (namely, electricity, heat, and light), in passing from the one order to the other, conserve their intrinsic characters, but they acquire others which are very different and altogether individually characteristic. These resemblances

and variations will be the subject of special subsequent study ; for the present, and in the first place, I ought to make known how gravitation, attraction, weight, density, cohesion, fusibility, solubility, and affinity are the immediate and necessary consequence of the state of vibration of the ponderable molecules of bodies reacting upon the elastic pressure of the imponderable medium which everywhere touches them.

Here the subject becomes so grave, it is of so much importance to science that the theory of universal gravitation should be understood, that I must prepare the minds of my readers by submitting to them material and sensible analogies and palpably verifiable facts.

If in the atmosphere, an elastic medium possessing at the same time expansibility and definite pressure, we conceive there to be two hemispherical cups of solid matter fitting on to one another exactly by friction, so as to form a sphere like that formed by the Magdebourg hemispheres, we may seek in vain to make them adhere to one another, that is, to give them mutual cohesion, if we do not have recourse to the well-known means of rarefying the air within the touching hemispheres.

As soon as we have, by the air-pump or some other means, diminished the interior pressure on the hemispheres, the two adhere. Their adhesion is small if but little air has been withdrawn ; they have a cohesion almost equal to the total pressure of the atmosphere if nearly all the interior air is removed.

Let us consider the case when the two hemispheres adhere by reason of the withdrawal of, let us say, a hundredth of the air which they contain. They may be separated in three ways :—(1) by applying to each hemisphere an opposite pulling-force equal to 10 grammes per superficial centimetre ; (2) by gently heating the sphere so as to restore to the air in it the hundredth of pressure which it lacks ; and (3) by placing the sphere under the receiver of an air-pump and exhausting the air in this receiver until the external pressure is equal to or less than the internal pressure. In each of these three cases the hemispheres will separate—in the first by *rending asunder* (*arrachement*) or overcoming the force of cohesion, in the second by *heat-fusion* (*fusion calorifique*), and in the third by *solution* (*dissolution*) in a fluid of equal pressure.

That which we see take place with two hemispheres we may obtain in the case of a cube formed of eight or twenty seven or other number of little hollow spheres opening into one another at their poles by holes whose edges are ground air-tight. On diminishing the interior pressure, which by its equality with the exterior pressure allows the spheres to move freely among one another, and putting them into exact contact at the moment

when their interior pressure is diminished, they will adhere together and present all the properties of a solid body. Further, if the little spheres were separated from one another by little tubular washers of caoutchouc between their communicating orifices, and if half the air were taken out of the spheres either by previous heating or by the air-pump, the imitation of the solid body so resulting would present all the appearances of contraction by cold, of expansion and fusion by heat; all the phenomena of extension by stretching or by rarefaction, of contraction by external pressure, and all those of destruction by mechanical rending, or by solution in a rarefied medium. Any instrument-maker could thus construct a solid body which would be elastic, expansible, dilatable, fusible, and soluble.

The approximation of the atoms of bodies of ponderable matter may therefore be due to the rarefaction of the imponderable fluid, and consequently to the diminution of its pressure in the space separating the atoms of the same body. And this approximation will be greatest and strongest accordingly as the interior rarefaction is greater, the exterior pressure being inversely as the interior. This hypothesis would be more than probable, it would be true, if it were shown that the vibration of the atoms of bodies may, and actually does, cause a rarefaction in the sphere of activity of each of the atoms.

If this proof could be given, we should be obliged to admit that attraction is a mechanical force composed (1) of the rarefaction of the æther in the interior of substances, of media, of heavenly bodies, brought about by the power of vibration of the atoms of ponderable matter; (2) of the reaction of the pressure of the exterior æther upon substances, media and heavenly bodies—a reaction measured by the general tension of the imponderable and incoercible fluid which constitutes the mother-liquor* of the world, the universal medium.

To gain a rational idea of the phenomena which take place in the physical world inaccessible to our direct means of observation, we must have recourse to analogies, to examples found amongst the class of phenomena which are visible, which are appreciable. I therefore said to myself thirty-seven years ago:—

If it be true that the atoms of ponderable matter on rapidly striking in their vibrations the ætherial elastic medium in a state of tension in which they are plunged rarely the æther around them and diminish its pressure in the spheres of their activity, and if this diminution of pressure results in the

* *Eaux mères.* The apparent grotesqueness is of course the fault of the translator; the translator, who is the editor, imagines the expression to be equally grotesque and admirable.—F. G.

attraction of other atoms, then it should also be true that a sonorous body acting on the air, on water, or other elastic medium will rarefy the fluid which envelopes it and attract to its surface bodies suspended or floating within the sphere of its activity. This question was neither proposed nor solved when I published my *Eléments de Physique Générale*. It was proposed and solved in 1832 and 1835, at the date of publication of my work on the movements of air and the pressure of moving air.

In 1832, during more than six months of varied and continuous experiments, the phenomenon refused to disclose itself to me; but I was convinced of its existence, and I should never have given up my search for it but with life. I had vainly tried the action of the tuning-fork upon all sorts of substances, when a pharmacist drew my attention to the pith of the Sunflower ("*grand soleil*") as a most light and bulky substance, and got me some of it. I made a little disk of this substance, which was attracted by the fork. It was attracted in all directions, and by all the faces of the vibrating instrument. It was even attracted by a needle connected with and forming a continuation of one of the prongs of the fork. Once master of the fact at first nearly imperceptible, it was not long before I reproduced it with ease and made it as palpable as magnetic attraction. I soon found that in order to get a proper action it was sufficient to expose to the vibrating surfaces a proportionally large surface in the pendulum subject to the influence. Rods, cups, cords, and plates in vibration attract, direct, or repel with the same appearances as electrified bodies. They act at a distance, and propagate their action through space to all surrounding bodies like electricity, heat, and light.

I take a tuning-fork and put it in vibration in the ordinary manner, I bring it near to a disk of vegetable paper hung from two silk threads without torsion. The disk is then seen to approach the surface from a distance of one or two centimetres, to touch the surface and to vibrate with it, and to detach itself as soon as by contact the vibration of the instrument is stopped. If the vibrating surface be increased by fastening a little card on to the external face of one of the prongs of the fork, the effects are produced from a greater distance, the pendulum is moved and attracted by the card, and remains in contact with it for a comparatively long time; and the distance of the attractions is proportional to the squares of the attracting and attracted surfaces*.

On passing the bow across the edge of a cup or of a bell of metal or glass, the same phenomena of attraction are shown

* It is to be regretted that this important point is stated with some ambiguity.—F. G.

opposite to the centres of oscillation, as experiment proves. But experiment also shows that no such attraction takes place opposite the nodes. There is therefore nothing in common between the attractions of the segments of vibration and the nodal lines traced out by light powders on plates, or exhibited by little paper saddles on a stretched cord. Electricity also has nothing whatever to do with these attractions, because they occur in metallic vibrating bodies as well as in those of glass, in water as in air. MM. Dulong and Savart, with whom I have repeated my experiments, have, moreover, conclusively shown that neither nodes of vibration nor electricity play any part in these attractions by sonorous vibrations.

In order better to fix the minds of my readers upon the true action of solid vibrating bodies on the elastic fluids which touch them, I pour some water into a bell-glass on a stem, and by means of the bow I extract the lowest sound which it can produce, one sees distinctly four segments in vibration, and four nodes which divide the liquid circle into four quarters.

If the bow be drawn gently along the edge of the vase without the slightest deviation, the segments of vibration appear formed of rays perpendicular to the surface of the glass, and which are longer accordingly as they are nearer to the centre of this segment. They are also of a length proportional to the intensity of the vibration. If the bow varies in position, the rays cross in all directions like the hatching of a drawing. If the vibration reach its maximum, the water is powdered and hovers in the form of a mist above the liquid. If the bell sounds the higher octave, there are eight segments of vibration, smaller, but presenting the same phenomena of radiation and projection of the water. In both cases the water remains at rest at the nodes and in the centre of the vase.

This experiment proves that the vibrations of solid bodies strike and forcibly disperse the elastic media in which they are plunged, that they rarefy these media in their sphere of action, and diminish their pressure more and more according to the proximity to their surfaces.

In order to show this fact still better, I hang a gold coin from two threads so that it cannot turn on its axis. I plunge it into the water opposite to a segment of vibration, at a distance of one or two centimetres from the interior surface of the vase, and on setting the vase in vibration by the bow, the coin is brought into contact with the glass. If the coin be opposite to a node it is not attracted.

I shall return later to these experiments, which I shall complete by showing that vibrating cords attract at one end and repel at the other, that a strip of paper hung in a bell always adjusts itself between two segments of vibration. Any one can

repeat the experiments (which I indicate in my work on the movements of air and the pressure of moving air), and will find them correct.

For the present I content myself with having proved experimentally that a body vibrating in an elastic medium reacts upon the medium in such a manner as to diminish its pressure at the place of contact and at a distance, the consequence of which is that bodies suspended in the medium and within the sphere of activity of the vibrating body are brought to the surface of the latter by exterior pressure, which overcomes the intermediate pressure between the attracting and attracted body.

That which occurs with vibrating bodies plunged in air ought to, and really does, occur in the case of atoms of coercible and ponderable matter plunged in æther, because no other explanation is possible of the phenomena of the formation of solid, liquid, and gaseous bodies, and of their transformation one into the other, of their elasticity, dilatibility, solubility (affinities of the tension of media and of heavenly bodies), of the weight of bodies, of gravitation—in one word, of universal attraction,—and because this explanation is in accordance with those phenomena which we can see and measure.

PART II.—*Acoustic Repulsion and Attraction*.

By K. H. SCHELLBACH*.—No. 1.

If the action exerted upon one another by atoms at exceedingly small distances be admitted as comprehensible, we must be prepared to admit the extension of the attractive or repulsive force to masses at very great distances from one another. The distant action of magnetism and electricity would then require no special explanation, and is not necessarily a consequence of the interposition of a connecting medium. Nevertheless it is possible that phenomena of attraction or repulsion at considerable distances might be occasioned by oscillations of æther or air.

Certain experiments which I performed last summer and first communicated to Herr Professor Quincke appear to support this supposition. I brought the flame of a stearine candle almost into contact with a tuning-fork fastened horizontally. As soon as the fork was set in vibration, the flame was quite distinctly repelled as long as the fork vibrated. When the flame was placed beneath the fork, it was depressed and flattened out into a disk. Resonance-disks and organ-pipes in a vertical position exhibited similar phenomena. A candle-flame at the mouth of a sounding-box provided with a tuning-fork which made 512 single vibrations per second, was strongly and continuously re-

* "Akustische Abstossung und Anziehung, von K. H. Schellbach," Pogg. *Ann.* vol. cxxxix. St. 4, p. 670.

pelled as long as the fork sounded. When the fork sounded loudly, the light was extinguished. A gas-flame a centimetre in length fed by a narrow glass tube, when placed at the opening of the sounding-box, split up into two tongues. Smoke rising from smoking candles was also repelled.

My earliest experiments showed that pith-balls hung on threads were attracted both by sounding tuning-forks and by vertical resonant plates. The above-mentioned sounding-box distinctly attracted and brought into contact with itself easily moveable metallic sheets and balls, even such as weighed 120 grammes and were at a distance of eight centimetres.

I communicated many of these experiments as early as the end of last Summer Session (*zu Ende des vorigen Sommerssemesters*) to Herr Geheimrath Magnus, who made them known to the young physicists who were working with him. Herr Professor Poggendorff also saw some of these experiments at my house. It seems, however, that all tuning-forks have not equal power to produce these phenomena; for when last autumn I wished to repeat the same experiments with other apparatus I failed.

Hitherto, in consequence of press of business, I have been prevented from continuing my experiments. Since, however, I see, from No. 115 of the Proceedings of the Royal Society of London, that Mr. Guthrie has performed similar experiments, I feel induced to make this preliminary communication.

Acoustic Repulsion and Attraction. By K. H. SCHELLBACH*.—
No. 2.

I ask permission to communicate the following experiments to complete those described in a previous Number of these *Annals*. (See above.)

Two glass flasks, each holding about 1 litre, were taken; a little ammonia was poured into the one, and hydrochloric acid into the other, so that their bottoms were covered to a depth of about two centimetres. Both flasks were provided with doubly bored corks. Atmospheric air from a gasometer was led into the ammonia-flask through a tube passing through the cork and reaching nearly to the surface of the liquid. Hence, through a connecting-tube, the ammonia entered the second flask, formed a cloud of sal-ammoniac which passed up a vertical tube having an upper orifice of about 2 millimetres, and rose into the air in a fine stream, 2 or 3 decimetres in height. Near to the opening of the exit-tube was placed the mouth of the sounding-box before described (see 1), the fork of which gave 512 single vibrations per second. When the rising gas-stream had a moderate (*mittlere*) velocity, it became divided as soon as the fork

* Pogg. *Ann.* vol. cxi. St. 2, p. 325.

sounded, into two almost straight currents which diverged from one another at an angle of about 30° , and which lay in a vertical plane perpendicular to the plane of the box's mouth. When the velocity is greater, the influence of the sonorous vibrations is still very perceptible, but the gas-stream does not appear so perfectly divided; but when the sal-ammoniac rises very slowly, there is produced either only a very weak repulsion, or else the effect of the sonorous pulsations is not to be distinctly detected at all. If hydrogen, coal-gas, or carbonic acid instead of atmospheric air, be led into the ammonia, the appearance is not essentially modified.

If the sal-ammoniac be made to pass through a heated glass bulb before it rises, no effect of the heating is seen at first. If the bulb be heated too strongly, the air-current becomes invisible on account of the sublimation of the sal-ammoniac.

In order to be able to examine the effect of heat, I led a current of atmospheric air over fuming nitric acid and allowed the gas to issue from a tube, upon the horizontal limb of which a bulb had been blown. The rising stream of nitrous acid, like that of sal-ammoniac, was also split into two parts, which were, however, not so distinctly separated as in the latter case. On heating the glass bulb, the effect of the sound-vibrations was no longer distinctly exhibited. All these phenomena could be recognized in the immediate neighbourhood of the sounding-box, and also when the gas-stream rose at a distance of one or two decimetres, or even further from it.

Besides the above-mentioned fork I also made use of a second one, which gave 1024 single vibrations per second, and could be screwed on to a resonance-box. A glass tube, 64 centims. long (that is, about as long as a sonorous wave produced by the fork), was fastened horizontally. If now the tuning-fork was set in vibration at one end of the tube, a light glass bulb (*Glaskugel*) or a disk was strongly attracted at the other end, and a candle-flame as distinctly repelled.

All these experiments, however, succeeded much better when the larger fork was used, which made 512 vibrations per second.

These and the previously mentioned phenomena were also produced, although to a less degree, when the tuning-fork was set in vibration by means of a second fork in unison with it. The actions were still visible when the distance between the forks was 1 metre.

The disks and light spheres with which the experiments were performed were fastened to the end of a brass needle about 2 decimetres in length, provided in the middle with an agate cap, and moveable like a magnetic needle around a vertical axis. If the needle be too long and the disks very light, the apparatus

becomes too sensitive to air-currents, so as to be useless for these experiments. An ordinary small magnetic needle, carrying on the one pole a paper disk of 4 centimetres diameter and on the other a small wax ball as counterpoise, is distinctly attracted by a vibrating tuning-fork from a distance of more than 1 decimetre.

The effect of sound-vibrations may be also detected on light and even somewhat heavy pendula. A ball weighing 3 kilogrammes hanging from a thread 2 metres in length could be set in visible vibration when the fork was stroked with the bow always isochronously with the pendulum's vibration. It was even possible in this manner to set in vibration an iron disk weighing 10 kilogrammes which could swing on a string 3 metres long fastened to an iron beam in the wall.

Quite recent experiments of mine have thrown somewhat more light on these phenomena. Two sheets of thin plate glass, 2 centims. long and 15 centims. broad, were fastened on to two stands and brought into the same vertical plane, so that a vertical slit 15 millims. wide was left between them. At a distance of 1 decimetre from the sheets of glass was placed the above-mentioned sounding-box with the plane of its mouth parallel to the sheets. If now a candle-flame was placed very near to the slit, between the slit and the box, it was as strongly attracted (drawn away from the slit) as it would otherwise have been repelled. If, however, the flame was placed on the other side of the box* rather near to the slit, it was repelled so as to be driven away again from the slit. This experiment explains at once the phenomenon presented by the current of sal-ammoniac. The rising column of vapour represents the slit, and the surrounding air corresponds with the rigid glass plate. Hence the stream is divided into two divergent branches as above described. It was to be expected that solid bodies might also in the same way be repelled by the sonorous vibrations in place of being attracted, as hitherto exclusively observed. In fact strips of stout cardboard, 15 millims. broad and 50 millims. long, are strongly repelled when they are placed at a distance of 5 to 8 millims. from the slit, between the plates and the box. Placed on the other side of the slit, they are attracted. The cardboard strips, therefore, approach the slit in both cases.

By the help of such a slit, which may be made by employing tinplate strips, the attractions and repulsions caused by sonorous vibrations may be produced at distances far greater than those hitherto noticed. They depend chiefly on differences in the

* "*Jenseits des Kästchens*." No figure is given; from the context I presume the expression should be "*jenseits des Spaltes*," on the side of the slit away from the box.—F. G.

densities and tensions of the masses of air upon which the vibrations act. When such differences exist only to a small degree (as, for instance, is the case with a very slowly ascending column of sal-ammoniac or of phosphorous acid rising from a fragment of phosphorus), the action of the sound-vibration is not sensible; it is just possible to perceive that, in the immediate neighbourhood of the phosphorus, a thicker mass of phosphorous acid is developed at every beat of the tuning-fork.

I am not at present able further to pursue these experiments, which may be extended greatly; and I should be very glad if they were taken up by those physicists who have chiefly occupied themselves with acoustic investigations.

In conclusion, I must mention that as early as 1834 M. Guyot noticed the attraction which light bodies suffer from a vibrating tuning-fork. He adds that this action extends to a distance of 9 lines. These observations are quite similar to those subsequently made by Mr. Guthrie*. Although a short notice of M. Guyot's experiments is to be found in *Pogg. Ann.* vol. xxxi. p. 640, I was not aware that such was the case till my attention was directed to it by others†. M. Guyot, in the *Presse Scientifique*, 1861, tom. iii. pp. 246-257, attaches very general theoretical reflections to his experiments, without, however, adducing new experiments upon which to base them.

My readers are already aware that Mr. Tyndall has already made use of ammonia gas in his beautiful experiments on sensitive flames.

Acoustic Repulsion and Attraction. By K. H. SHELLBACH‡. No. 3.

The sonorous vibrations of an elastic medium attract specifically heavier bodies to the centre of disturbance and repel specifically lighter ones.

In order to establish this theorem experimentally, a small balloon of goldbeater's skin, 2 decimetres in height, was filled with atmospheric air and hung on a thread. A second similar balloon filled with hydrogen was fastened by means of a thread to a table so as to hinder it from rising. As soon as the above-mentioned tuning-fork was set vibrating at a distance of 1 or 2 centims. from these balloons, the first approached the sounding-box till it touched it, the second was violently repelled. This repulsion also took place when the top of the balloon was fas-

* Of course, when writing this, Herr Shellbach was only cognizant of the abstract of my experiments in the Proceedings of the Royal Society, No. 115, and was not aware of their scope.—F. G.

† I also was ignorant of M. Guyot's experiments and conclusions. A translation of them forms Part I. of the present collection of papers on the subject.—F. G.

‡ *Pogg. Ann.* vol. cxl. St. 3, p. 495.

tened by a thread to a stand, and the bottom so loaded with wax fastened to it by a string, that the whole system had a tendency to sink.

In order to test the theorem further, a large glass cylinder was filled with carbonic acid, and one of the above-mentioned balloons was filled partly with atmospheric air and partly with illuminating gas, so that it floated in the carbonic acid and only exposed its extreme top above the cylinder. The vibrating fork distinctly repelled this balloon.

Hitherto I have not yet had time to examine the effect of the before-mentioned slit upon this system (see 2).

I may be allowed to add that the influence of such a slit was exhibited at a distance of 6 decimetres.

The majority of the above-described experiments may, moreover, be performed with a glass bell-jar such as is commonly used for domestic purposes, and with an ordinary easily moveable magnetic needle.

A little machine for striking, the hammer of which is set going by an electric current, shows the attraction of a strip of paper in a very regular manner; so that when the machine has been at work for some minutes, the paper is brought into contact with the bell.

The above observations furnish a sure means for the explanation of the phenomena offered by the so-called sensitive flame.

PART III. *Letters from Professor Sir W. Thomson to Frederick Guthrie.*

Glasgow, Nov. 14th, 1870.

DEAR SIR,—I have to-day received the ‘Proceedings of the Royal Society’ containing your paper “On Approach caused by Vibration,” which I have read with great interest. The experiments you describe constitute very beautiful illustrations of the established theorem for fluid pressure in abstract hydrokinetics, with which I have been much occupied in mathematical investigations connected with vortex-motion.

According to this theorem, the average pressure at any point of an incompressible frictionless fluid originally at rest, but set in motion and kept in motion by solids moving to and fro, or whirling round in any manner, through a finite space of it, is equal to a constant diminished by the product of the density into half the square of the velocity. This immediately explains the attractions demonstrated in your experiments; for in each case the square of the average velocity is greater on the side of the card nearest the tuning-fork than on the remote side. Hence obviously the card must be attracted by the fork as you have found it to be; but it is not so easy at first sight to perceive

that the square of the average velocity must be greater on the surfaces of the tuning-fork next to the card than on the remote portions of the vibrating surface. Your theoretical observation, however, that the attraction must be mutual, is beyond doubt valid, as we may convince ourselves by imagining the stand which bears the tuning-fork and the card to be perfectly free to move through the fluid. If the card were attracted towards the tuning-fork, and there were not an equal and opposite force on the remainder of the whole surface of the tuning-fork and support, the whole system would commence moving, and continue moving with an accelerated velocity in the direction of the force acting on the card—an impossible result. It might, indeed, be argued that this result is not impossible, as it might be said that the kinetic energy of the vibrations could gradually transform itself into kinetic energy of the solid mass moving through the fluid, and of the fluid escaping before and closing up behind the solid. But “common sense” almost suffices to put down such an argument, and elementary mathematical theory, especially the theory of momentum in hydrokinetics explained in my article on “Vortex-motion,” negatives it.

The law of the attraction which you observed agrees perfectly with the law of magnetic attraction in a certain ideal case which may be fully specified by the application of a principle explained in a short article communicated to the Royal Society of Edinburgh in February last, as an abstract of an intended continuation of my paper on “Vortex-motion.” Thus, if we take as an ideal tuning-fork two globes or disks moving rapidly to and fro in the line joining their centres, the corresponding magnet will be a bar with poles of the same name as its two ends and a double opposite pole in its middle. Again, the analogue of your paper disk is an equal and similar diamagnetic of infinite diamagnetic inductive capacity. The mutual force between the magnet and the diamagnetic will be equal and opposite to the corresponding hydrokinetic force at each instant. To apply the analogy, we must suppose the magnet to gradually vary from maximum magnetization to zero, then through an equal and opposite magnetization back through zero to the primitive magnetization, and so on periodically. The resultant of fluid pressure on the disk is not at each instant equal and opposite to the magnetic force at the corresponding instant, but the average resultant of the fluid pressure is equal to the average resultant of the magnetic force. Inasmuch as the force on the diamagnetic is generally repulsion from the magnet, however the magnet be held, and is unaltered in amount by the reversal of the magnetization, it follows that the average resultant of the fluid pressure

is an attraction on the whole towards the tuning-fork, into whatever position the tuning-fork be turned relatively to it.

Your seventh experiment* has interested me even more than any of the others. It illustrates the elementary law of pressure in hydrokinetics, not by showing effects of fluid pressure on portions of a solid bounding surface, as all other illustrative experiments hitherto known to me have done, but by showing an effect of diminished fluid pressure throughout more rapidly moving portions of the finite mass of the fluid itself. This effect consists of a slight degree of expansion, depending on the air not being perfectly incompressible. The volume occupied by the more rapidly moving portions becoming slightly augmented, the remainder of the fluid would be condensed were the whole contained within an altogether fixed boundary. A moveable portion of this boundary (that is, the surface of the liquid in your tube) yields and shows to the eye the effect of the diminished pressure through the rapidly moving portions.

No branch of abstract dynamics has had a greater charm for the mathematical worker than hydrokinetics; but it has not hitherto been made generally attractive by experimental illustrations. Such refined and beautiful experiments as those you describe, and especially your seventh, tend notably to give to this branch of dynamics quite a different place in popular estimation from that which it has held; but what is perhaps of even more importance, they help greatly to clear the ideas of those who have made it a subject of mathematical study.

Yours truly,

Professor Guthrie.

WILLIAM THOMSON.

Nov. 16, 1870.

DEAR SIR,—On writing to you on the 14th I forgot to remark that the dynamical theory demonstrates the truth of your prediction in § 18†. It shows that the amounts of the expansions of different gases in equal and similar arrangements exhibiting your 7th experiment‡ are simply as their densities, it being assumed always that the whole amount of expansion is such as to produce only an infinitesimal change of density in any part of the gas.

I remain, dear Sir,

Yours truly,

Professor Guthrie.

WILLIAM THOMSON.

* *Experiment 7* in *Proceedings Roy Soc.* vol. xix. p. 38, or *experiment 10*, *Phil. Mag.* Nov. 1870.—F. G.

† *Proceedings Roy. Soc.* No. 123, § 18. *Phil. Mag.* Nov. 1870, § 27.

‡ *Proceedings Roy. Soc.* No. 123, *Ex.* 7. *Phil. Mag.* Nov. 1870. *Ex.* 10.

Nov. 23, 1870.

DEAR SIR,—The repulsion of a hydrogen balloon observed by Shellbach, of which you tell me in your letter of the 16th inst., is temptingly suggestive of the conclusion you tell me he has drawn, that there is attraction or repulsion accordingly as the density of the interior gas is greater or less than that of the surrounding air. There are, no doubt, curiously close analogies between some of the circumstances of motion in contiguous fluids of different densities and the distribution of magnetic force in a field occupied by substances of different inductive capacities. Thus, if in a great space occupied by frictionless incompressible liquid denser in some portions than in others, a solid be suddenly set in motion, the lines of the fluid motion first generated agree perfectly with the permanent lines of magnetic force in a correspondingly heterogeneous medium under the influence of a bar-magnet, to be substituted for the moveable solid and placed with its magnetic axis in the line of the solid's motion. As to amounts, the fluid velocity multiplied into the density is simply equal to the resultant magnetic force at each point, if the particular definition of resultant magnetic force in a medium of heterogeneous inductive capacity, given in the footnote to § 48 of my paper on the "Mathematical Theory of Magnetism"* be adopted. But here the analogy ends; the rigidity in virtue of which a solid moveable in a fluid medium differing from it in magnetic inductive capacity keeps its form, does not exist in the hydrokinetic analogue. If a quasi-rigidity be given to a globe of lighter fluid, as a hydrogen soap-bubble, by the tension of a containing film, or to a hydrogen balloon by the stiffness and tension of the containing membrane, the circumstances become so complex that no general dictum can be readily pronounced regarding the resulting force from the abstract dynamical theory. It is certain that if the tension of the containing film were sufficiently great even without rigidity, or if the containing membrane were sufficiently stiff by rigidity, as a thin shell of solid, the balloon would be attracted as a perfectly rigid globe quite irrespectively of the density of its contents.

But it seems, from Shellbach's results, that the membrane of the hydrogen balloon may have stiffness *enough* to keep the figure very approximately spherical, but yet *not too much* to allow the smaller density within to exercise its influence and to give attraction instead of repulsion†. The question is highly

* Philosophical Transactions, June 21, 1849. Published in Part I. for 1851.

† [Note added 13th April, 1871.] The details of Schellbach's experiments described in the preceding article, which were unknown to me when

interesting as a mathematical problem, which will no doubt prove tractable under certain assumed conditions as to tension or rigidity of the containing film.

Yours very truly,
 Professor Guthrie. WILLIAM THOMSON.

Nov. 24, 1870.

DEAR SIR,—My letter to you of yesterday requires some limitation.

Even a perfectly rigid solid less dense than the surrounding medium must, if small enough, exhibit the repulsion which you tell me Shellbach has observed for balloons of hydrogen. On the other hand, if large enough, it is clear that, as I said yesterday, a rigid solid must exhibit attraction; for if very large relatively to the vibration, it cannot move sensibly as a whole to and fro with each vibration. Now, if the rigid body is held absolutely fixed, it follows from your own experiments and reasoning, as well as from the mathematical theory, that the vibrator will experience attraction, irrespectively of the density of the fixed rigid solid; and therefore when the vibrator is attached to a fixed stand and the rigid solid left free to move, the latter, if large enough, must experience repulsion. Consider, first, any very small portion of the fluid moving to and fro in virtue of the periodic motion sustained throughout the fluid by the vibrator. Let it be so small in all its dimensions that it experiences no sensible change of shape during its period of motion. If this portion becomes rigid its rigidity will but infinitesimally affect its own motion and that of the fluid surrounding it. If, now, by a proper application of force varying periodically according to the simple harmonic law, the little solid be caused to move to and fro faster than the neighbouring fluid or slower than the neighbouring fluid, it will require a certain constant average force to prevent its mean position from yielding in a certain direction from or towards regions of greater fluid motion. When the superimposed vibration augments the natural vibration of the solid, the direction in which it tends to move is from regions of greater towards regions of less fluid vibration; in the other case the contrary. Lastly, the imagined differential vibration becomes a reality if the density of the little solid is either greater or less than that of the fluid. When it is less than that of the fluid, the differential motion augments the vibration; when the density is greater, the differential motion diminishes the vibration. Hence an infinitesimal solid of different density from the surrounding fluid tends towards the re-

I wrote the letter in the text, render it, I think, quite certain that the explanation here suggested is correct.—W. T.

gions of greater fluid motion or of less fluid motion (that is to say, is attracted towards the vibrator or repelled from it) accordingly as the density is greater or less than that of the fluid.

I have worked out this theory in detail for a perfectly rigid spherical solid left to itself in an incompressible liquid disturbed by a single spherical body moving to and fro with a simple harmonic motion through a short space in a line with the free sphere, the radius of one at least of the two spheres being so small that there is no sensible disturbance of the fluid in the neighbourhood of the free sphere due to the influence of this body itself reflected back to the vibrator. When the density of the free sphere differs from that of the liquid, I suppose there to be no force of gravity. In illustrative experiments, when the force of gravity is sensible, it may be readily balanced by means of a thread properly applied to support the body or to prevent it from rising, as the case may be, in general without sensibly interfering* with the to-and-fro motion of the sphere. The range of the vibration may be of any magnitude relatively to its own diameter, but must be small relatively to the distance of the nearest point of the free globe. A very simple application of the method of images suffices for the problem. I intend to give the details in the continuation of my paper on "Vortex-motion." One chief result is this: when the density of the free globe is less than that of the fluid, it is repelled or attracted, according to whether its radius is less or greater than

$$f \cdot \sqrt{1 - \sqrt[5]{\frac{1+2\rho}{3}}},$$

where ρ denotes the density of the free globe, and f its distance from the vibrator (the density of the fluid being taken as unity). From this it follows that the free globe, if of great diameter in comparison with that of the vibrator, is repelled when placed in the line of the vibration at any distance greater than

$$\frac{r}{\sqrt{1 - \sqrt[5]{\frac{1+2\rho}{3}}}},$$

and is attracted when placed at any distance less than this cri-

* [Note added April 13th, 1871.] Not so readily as I believed when writing the above. Even a rough approximation to this condition is not very "readily" attained; and on this account experimental verification of the theoretical conclusion is somewhat difficult. I have, however, obtained more or less imperfect illustrations of it readily enough by using the hand or a round wooden bar to produce vibratory motions in the neighbourhood of wooden balls or other light rigid globes anchored by very thin threads, allowing them to float near the surface of water in a vat about 6 feet deep.

tical value. The critical distance is one of unstable equilibrium. For the case of $\rho=0$ (that is to say, the free rigid body a massless sphere) the critical distance is

$$\frac{r}{\sqrt{1 - \sqrt{\frac{1}{3}}}}, \text{ or } 2.251r;$$

that is, very approximately $2\frac{1}{4}$ radii of the free globe.

Yours truly,

Professor Guthrie.

WILLIAM THOMSON.

11th Jan. 1871.

SIR,—I am desired by Sir W. Thomson to send you a description of the following experiment on “Approach caused by Vibration.”

A light collodion balloon is filled with hydrogen, and the thread which closes the orifice is prolonged to a length of about 40 inches. To the free end of the thread a light piece of wood is tied to serve as a floater. The latter is now put upon the surface of water and serves to moor the balloon, giving it at the same time freedom of motion in all directions. If, now, a sheet of tin-plate (or, better, a light wooden board with rounded edges) is held perpendicularly in a line with the centre of the balloon and vibrated to and fro at a distance of several inches from it, the balloon approaches the vibrating plate*, drawing with it the floater over the surface of the water. Vibrations of the balloon isochronous with those of the plate are also observed.

I am, Sir,

Your obedient Servant,

Professor Guthrie.

JOHN TATLOCK.

LVI. *On Barometric Differences and Fluctuations.* By JOHN KNOX LAUGHTON, M.A., F.R.A.S., F.R.G.S., *Mathematical and Naval Instructor at the Royal Naval College, Portsmouth.*

[Continued from p. 350.]

PART III. *The Barometric Depression in Storms.*

MANY years ago Admiral Fitzroy pointed out that storms are properly to be regarded merely as deviations from the ordinary and regular movements of the atmosphere, and that, violent as is their force, disastrous as are their effects, they constitute in reality a very small part of that motion of the air which is continually going on, and which the study of prevailing winds

* Compare with Herr Schellbach's experiments, *sup.* p. 422, who, on using a tuning-fork of 512 single vibrations per second, obtained repulsion.—F. G.

can alone make us satisfactorily acquainted with. Independent of the great weight due to Admiral Fitzroy's opinion, there are many considerations which appear to sanction this view to the fullest extent, and to render it imperative that a correct knowledge of the seasons, the directions, and the characteristics of the prevailing winds in the different parts of the world should form the basis of any inquiry into the origin of storms: it is their exceptional nature which has to be inquired into; and the regular course to which they are exceptions must first be clearly understood. Meteorologists have, as a body, ignored this necessity, and, in treating of storms, have affected to consider the air as in a state of rest, and disturbed from that state by the sudden development of some agency which destroys the hydrostatic equilibrium. But, in point of fact, the normal condition of the air is not one of hydrostatic equilibrium: in almost every part of the world, and at every season, some particular wind has a decided predominance over every other; and wherever storms occur, they follow the course of the prevailing wind of the locality and season. The exceptions to this rule are extremely rare; and admitting that storms are most commonly of the nature of cyclones, I can see in the law which directs their track no difference from that which directs the onward motion of the angry eddies which form in a rapid stream. And in the same way as such whirls generally form in particular places, where we can often point out the cause of the irregularity (as, for instance, where another stream shoots in its waters across the course of the main current, or where a boulder or some partial obstruction breaks the evenness of the flow), so also do storms form most frequently in special localities, in almost every one of which we can point distinctly to the near approach of intrusive currents, or to the headlands, islands, and opposing shores round and against which the wind fumes and rages. Whether we consider the tropical Hurricanes (taking their rise on the equatorial margin of the Trade-Winds or in the conflict of changing Monsoons), the storms of the North Atlantic (where the northerly gales from the coasts of Greenland are driven into the body of the west wind), the storms off Cape Horn or the Cape of Good Hope (the Cape of Storms), or even those off Cape de Gata, or Cape Matapan (famous in story), off Bermuda with its countless number of islands, or the Abrolhos, or the Low Archipelago, or in any other locality where storms are notoriously frequent and violent,—we find everywhere, or with very few exceptions, some marked feature, either of the prevailing wind in the stormy season, or in the geographical peculiarities of the land, which seems directly to associate itself with the irregularities in the aerial current.

It is this geographical distribution which leads me forcibly to

the conclusion that storms are due in great measure to the geographical conditions under which they occur, and that, though possibly originated by meteorological disturbances, the part played by these must be comparatively trifling, since in other localities, where similar disturbances must frequently occur, they do not give rise to similar storms.

If, then, storms of a cyclonic nature (and it is to these only that I here refer) are caused by the mechanical clashing of atmospheric currents either against each other or against more material obstacles, it is evident that the remarkable barometric depression in their centre is, in the first instance, occasioned by the force of the whirl, and that the whirl is not occasioned by the barometric depression. This, however, is contrary to the opinion now in favour with many meteorologists, who maintain that the barometric depression is first formed, and that the storm, or the rush of air towards the area of minimum pressure, is more or less violent according to the extent to which the pressure has been reduced. That there is a very intimate connexion between the violence of the storm and the depression of the barometer would seem to be certainly established; but they are so mixed up one with the other, each operating on and increasing the other, that it is difficult to assign to each its exact value as agent and as patient. But this is not what is meant by those who teach that the barometric depression is the absolute cause of the storm, and that the depression is due to other agencies, the capabilities of which they seem to me to overrate.

Of these agencies, that on which most stress is laid is the condensation of aqueous vapour, and the sudden manifestation of heat in the upper regions of the atmosphere. That excessive rain is the invariable concomitant of storms of this class, and more distinctly of tropical hurricanes, is not to be disputed; but there are many places in the world where rain falls at times quite as freely as in the very centre of a hurricane without producing any depression such as is here spoken of. I have already examined into the effect of heavy rain on the barometer in India and other places; but I nowhere find any grounds for assigning to it a capability of diminishing the pressure by 2·5 inches, a diminution which has been observed in the centre of a cyclone; neither are the localities in which cyclones take their rise localities of excessive rain. If the aqueous vapour in these districts is not liable to free condensation, and if the very freest condensation which we have satisfactory means of measuring, as at Cherrapongee, produces no such excessive effect on the barometer—no effect, indeed, at all approaching to it—it is difficult to admit that condensation is the cause of the depression observed. The whole of the depression, however, is not attributed to this;

it is asserted that, by reason of the comparative vacuum formed high overhead, the air rushing in forms an ascending current, the centre of depression becomes a species of chimney up which the air is carried with great velocity, and that this ascensional motion accounts for the full extent of the loss of pressure. But any ascending current so formed is due directly to the condensation, and must occur wherever condensation is excessive; any depression of the barometer caused by it is essentially a component part of the depression caused by heavy rain, and cannot be separated from it in any observations. Since, however, considerable stress has been laid on the influence of this ascensional motion, it is worth while to inquire into how far it really exists, and what effect it is really capable of producing on the barometer.

It has always been a favourite opinion amongst meteorologists that the ventilation of coal-mines by means of furnaces offers a convenient and apt illustration of the movements, and the causes and effects of the movements, of the atmosphere. The correctness of this opinion is, I think, more than doubtful: I have already called attention to the fact that the differences of temperature produced in the furnace method of ventilation are, beyond all legitimate comparison, greater than any that occur in nature; and not only these excessive differences of temperature, but also the friction of the currents of air driven at a high velocity through narrow and winding passages, or what is technically called "the drag of the air," produce a very marked effect on the pressure as shown by the barometer or gauge, which must be eliminated before we can tell how much of any registered difference between the air of the upcast and downcast shafts is due to the ascending or descending motion of the current. Captain Toynbee has recorded that, observing with an aneroid, he "found a difference of a tenth of an inch between the pressures at the bottom of the downcast and upcast shafts, both positions being on the same level, whilst halfway down each shaft there was only a difference of four hundredths of an inch"*. By the kindness of Mr. Landale, of Lochgelly, I am enabled to present the results of some similar observations which he has recently had taken at my request. They are as follows:—

Name of pit.	Number of observations (double).	At a depth in fathoms.		Mean reading of aneroid.	
		Upcast.	Downcast.	Upcast.	Downcast.
Lady pit	6	70	70	30.190	30.215
Foulford	7	70	71.5	30.316	30.350
Pit No. 21	4	24.5	25.5	29.788	29.848

* *Meteorology of the North Atlantic*, p. 12.

Of these, Lady pit shows a difference of $\cdot 025$; when the Foulford observations are reduced by the correction for 9 feet, they show a difference of $\cdot 026$; and at pit No. 21, where the top of the upcast shaft is 36 feet higher than that of the downcast, so that a correction for 42 feet has to be applied, there is a difference of $\cdot 014$.

These observations are quite in accord with those of Captain Toynbee, and show beyond question a certain small relative depression in the upcast shaft which might at first sight be attributed to the upward motion, as opposed to the downward. But we must not forget that between the columns of air in the two shafts there is a very great difference of temperature and of weight; the elastic force derived from the pressure of this weight is therefore less in the upcast than in the downcast; and before we can properly assign the observed barometric difference to the difference of motion, we must show that it is independent of the difference of weight. The following Table, which I collect from the paper by Mr. Wood already referred to, seems sufficient to set this point at rest*.

Observations at Killingworth, the mean temperature of the downcast being 49° , and of the upcast 147° , the sum of the velocities 45.8 feet per second.

Depth, in fathoms.	Water-gauge.		Difference of weight of column, pounds per square foot.
	Inch.	Pounds per square foot.	
10	$\cdot 10$	$\cdot 521$	$\cdot 769$
20	$\cdot 115$	$\cdot 598$	1.538
30	$\cdot 13$	$\cdot 677$	2.308
40	$\cdot 20$	1.041	3.076
50	$\cdot 25$	1.302	3.845
60	$\cdot 35$	1.822	4.614
70	$\cdot 45$	2.344	5.383
80	$\cdot 57$	2.968	6.152
90	$\cdot 70$	3.645	6.922
100	$\cdot 78$	4.062	7.690
110	$\cdot 74$	3.854	8.460

Killingworth pit, in which these observations were made, had the upcast and downcast in the same shaft, separated from each other by an air-tight wooden partition or brattice; in this, at the several heights, holes were bored, through which one leg of the water-gauge (a U-shaped glass tube) could be passed. So taken, and with an instrument so delicate, they show very clearly that the force of the currents has very little, if any thing, to do

* Experiments &c. pp. 20, 81, and plate 8.

with the difference in pressure which is registered. If a certain number of cubic feet of air are each minute driven up one shaft or down another, the velocity of the ascending or descending current must be approximately the same throughout the shaft at all depths, and the influence which the motion exercises on the barometer or gauge must be constant. That the observed difference, far from being constant, increases as the depth increases, shows at once that it is not to the motion that we must look for its cause, but rather to the difference of the weight of the columns of air. But the difference shown by the gauge is much less than what is due to the difference of weight at the several depths, calculated for the mean temperatures: near the top this is partly to be accounted for by the temperature of the upcast being lower than the mean, and the remaining portion of the column being thus heavier than is calculated; but near the bottom, where the temperature is higher than the mean, it would seem that expansion does not take place quickly enough to balance the increase of elastic force caused by the heat. A large part of the pressure due to the difference of weight is absorbed by the "drag of the air;" it is the remainder only which forces the heated air up the shaft; and the velocity is thus much less than what is properly due to the difference of temperature. It is, however, impossible to say how much of the relative increase of elasticity is due to heat, how much to mechanical compression; but near the top of the shaft, where the weights of the columns become the same and the compression ceases, though the velocity remains necessarily unchanged, the gauge no longer shows any difference.

It appears to me, therefore, that, so far as we can apply the circumstances of coal-pit ventilation, there is absolutely no evidence whatever that any diminution of barometric pressure is caused by an ascending current, or, conversely, any increase by a descending one. The belief in such an effect appears, in fact, to be due to the confusion between weight and elastic force, already spoken of; for where such a confusion of idea exists, it is natural to suppose that an ascending weight will exert a less pressure than a stationary or a descending one.

But even if we were to admit that a barometric difference may be due to such a cause, to the fullest extent that can be claimed for it, it would still be quite impossible to attribute to it any appreciable decrease of pressure in the centre of a cyclone. Mr. Buchan conceives that in a cyclone the air rushing towards the area of least pressure rarely makes a complete circuit*; and since the force of the wind is greatest in the immediate neighbourhood of the central calm, it follows, according to this view, that air with its maximum velocity is entering into the central

* Handy Book, p. 281.

space along its entire circumference. In violent cyclones the maximum velocity of the wind is estimated as certainly not less than 120 miles an hour, and is probably a good deal more. The height to which the storm extends is absolutely unknown; Captain Maury has estimated the height of the Trade-Winds as about three miles; I should be inclined to agree with Redfield, that one mile is probably the extreme height of a cyclone. If, then, the diameter of the central space be 12 miles, the quantity of air that is poured in in the course of an hour is $\pi \times 12 \times 120$ cubic miles, which, disappearing upwards from an area of $\pi \times 36$ square miles, gives us an ascensional velocity of 40 miles an hour—that is, of 66 feet a second. Even then, if we were to suppose that the greatest difference observed at Killingworth, which is equivalent to a barometric depression of $\cdot 06$ of an inch, corresponds to the aggregate velocity of 46 feet a second, and that the depression increases in proportion to the velocity, we should still have no more than $\cdot 09$ of an inch as the measure of that component of the depression in the centre of a cyclone which could be attributed to this cause. But such a supposition would be wild, to the verge of absurdity; and not less so is the estimate of 66 feet a second as the velocity of the ascending current. Such a velocity represents itself a gale of wind, a wind of force 7 on the Beaufort scale; and most certainly nothing like it was ever observed in the most violent cyclone. That the air towards the centre of these storms has a sensible upward motion has been noticed; but even an approximation to such a velocity as 66 feet a second is out of the question. A velocity of 5 feet a second, or even 10, may perhaps be admitted; and if, for the sake of the argument, we were to allow that the upper difference observed at Killingworth, or $\cdot 10$ of an inch of the water-gauge, denotes the depression caused by a velocity of 46 feet, we might so account for a central barometric depression of about $\cdot 0015$ of an inch, a depression which no instrument practically used at sea could be depended on to register.

If, then, the barometric depression in the centre of a cyclone cannot be referred to condensation, if it is not due either to the withdrawal of the elastic force of the aqueous vapour or to the liberation of heat above, if it is not due to the ascensional velocity of an upward current, meteorological reasons for its existence fail us, and we are thrown back on that which I have already suggested as its primary cause—the centrifugal tendency established by the force of the whirl itself. This is constantly said to be utterly inadequate to produce such a depression as is observed; as I have just shown the insufficiency of other alleged causes, I will attempt to form some estimate of what may be the capability of this mechanical agent.

We know that if a vessel containing fluid be made to rotate uniformly round a vertical axis with an angular velocity ω , the surface or the strata of equal pressure of the fluid assume, as a position of equilibrium, the form of a paraboloid of revolution, the *latus rectum* of which is $\frac{2g}{\omega^2}$, and that, by the ordinary equation to the parabola, if d denote the depression of the lowest point or vertex, below any other point whose distance from the axis of rotation is r ,

$$d = \frac{\omega^2 r^2}{2g};$$

or if v be the linear velocity of the point (r, d) ,

$$d = \frac{v^2}{2g}.$$

It is evidently impossible to expect a simple formula such as this to apply with correctness to the motion of the air in cyclones; for since the air rotates independently and grinds on all sides, whether against the surface of the earth or non-rotating air, the friction must effect very great changes; the linear velocity, instead of increasing as the point recedes from the centre, diminishes; and the velocity at the surface, which we can observe, is very much less than it is at some considerable height, though we have very scanty means of ascertaining what the difference may be. The balloon which landed near Christiania on the 25th of last November, travelled from Paris at the rate of from 40 to 45 miles an hour, whilst the wind below had a velocity of not more than 20. The differences noted by Mr. Glaisher in his several ascents are still greater: counting only the distance actually made good by the balloon, the mean of nine experiments gave 25 miles an hour as the velocity above, whilst the anemometer showed 7 miles an hour as the velocity below*. We cannot say how far this may be considered as representing the ratio between the upper and lower velocities in cyclones or other furious storms: we have simply no evidence; and when the surface velocity is 120 miles an hour, the mind almost refuses to conceive a storm of such tremendous power as would be implied by a velocity of even 240 miles an hour. Still, if we take the extreme velocity of the wind observed near the centre as fairly representing the surface velocity of the whirl free from lateral friction, and twice that extreme as representing the upper velocity free from horizontal friction, we find that the depressions so calculated by this simple formula have a very remarkable coincidence with those which have been observed.

* Travels in the Air, p. 91.

Modifying the formula in accordance with the customary unit, let d denote the depression of the aerial strata of equal pressure at the vortex, in feet ;

v , the velocity of the wind in miles per hour ;

a , the velocity in feet per second ;

then

$$a = \frac{6000}{3600} v = \frac{10}{6} v,$$

and

$$d = \frac{a^2}{2g} = \frac{100}{36 \times 64} \cdot v^2 ;$$

so that if δ denote the barometric depression in inches, we have

$$\delta = \frac{d}{1000} \text{ nearly,}$$

or

$$\delta = \frac{v^2}{36 \times 640} ;$$

and arranging the results so obtained in a tabular form, we have :—

$v.$	$\delta.$	$v.$	$\delta.$	$v.$	$\delta.$
60	·156	120	·625	190	1·566
70	·212	130	·733	200	1·736
80	·278	140	·850	210	1·914
90	·351	150	·976	220	2·100
100	·434	160	1·111	230	2·296
110	·525	170	1·254	240	2·500
		180	1·406		

The uepressions here tabled agree very closely with those which have been observed, not only in cyclones, but in comparatively moderate storms—as, for instance, in that which will be so sadly remembered as “the Captain’s gale,” off Cape Finisterre, on the 7th of last September, when, about 1^h 30^m A.M., the wind, having a force 9 on the Beaufort scale, corresponding to a velocity of about 60 miles, suddenly shifted from S.S.W. to N.N.W., the barometer at the time standing at 29·32, thus showing a depression of ·68 of an inch. But though such an agreement is far closer than could have been anticipated, and seems to leave little room for doubt that it indicates this centrifugal tendency as the first cause of the depression, I would not be understood to imply that the whole depression is due to it. Twice the greatest observed velocity is probably an exaggerated estimate ; and a considerable reduction may readily be allowed, since to whatever depression is produced by the centrifugal mo-

tion a large addition must be made on account of the excessive condensation that takes place.

It is very commonly objected to the view which I have here formed of the effect of centrifugal motion, that if the depression were so caused, the tendency of the air being to fly outwards, the wind would necessarily tend outwards, whilst careful observation shows that in such storms the wind, on the contrary, tends inwards*. That the wind does frequently tend inwards is a well-established fact; but the objection is founded on an incorrect idea of the motion which results from a centrifugal tendency under the peculiar circumstances. The air whilst rotating is held in by the elastic force and weight of the comparatively quiescent air outside the whirl, the friction against which checks its motion; it cannot fly off as water from a twirling mop; it is confined within close limits; but as the velocity above is very much greater than that at the surface, the air is there able to force its way much further, and to make a section of relative low pressure in the column of low pressure. Towards this the air from the surface forces itself, and generates an ascending current; towards the place left vacant by the ascending air other surface air moves and forms this vorticoise movement, which is to be observed not only in cyclones and storms, but in the dust whirlwinds of every arid plain, in the little whirls of sand and straw at the corner of a street on a March day, and may be shown experimentally in a tumbler of water with a few grains of sand in it; if the water be stirred briskly round, the vorticoise movement which goes on at the bottom will be distinctly visible, and the sand will finally arrange itself in a sharp-pointed conical heap in the middle. The vorticoise movement is a distinct, clearly marked, and invariable consequent of a centrifugal tendency manifesting itself in a fluid revolving in a confined space; and its presence in a cyclone would be very far from invalidating the opinion I have just stated, even if the excessive condensation did not in itself produce a sufficient depression to meet the objection.

Independently of the arguments I have already adduced, there are several phenomena exhibited by storms which appear to me capable of ready explanation when referred to the mechanical action of the wind, but are exceedingly difficult to understand if that action is denied: these have been observed principally in tropical cyclones, where, the violence being greatest, irregularities are, as it were, magnified, and rendered less likely to escape notice. Such a phenomenon is the rise in the barometer which is very frequently found on the outskirts of a hurricane, and which would seem due to the outward pressure of the air

* Handy Book of Meteorology, p. 280.

endeavouring to fly from the centre of rotation ; far beyond the limits of the storm the air is thus compressed, and has its elastic force disturbed to an extent that is at once noticed as breaking the general regularity of barometric readings within the tropics*.

A still more remarkable phenomenon is the formation of one or several smaller whirlwinds in the body of the larger one. The wind pressing onwards in confused violence, and being unable to drive some slower-moving mass before it, is turned towards the centre of lowest pressure and thrown into a whirl which rotates in the same direction as the parent storm, but in which the force and velocity are intensified to an extreme degree†. The velocity is indeed so great that the ascensional motion generated is frequently communicated even to the water of the sea, which rises in vertical and rapidly whirling columns ; these travel, for the most part, with the wind of the hurricane, carried in its circling course along whichever quarter they are formed in ; but in exceptional cases they have been known to travel in the same direction as the body of the storm, irrespective of the particular and temporary wind at the place where they originate. And the constancy with which cyclones or revolving storms in every known locality follow a specified track from which they rarely deviate, such tracks leading in different parts of the world to very different points of the compass, but always in the direction of the prevailing wind, seems to me the phenomenon which, more almost than any other, establishes the value of what I would call the mechanical, in contradistinction to the meteorological agencies. This, however, has been overlooked by meteorologists, who, misled by the name North-East Trade in the North Atlantic, or South-East Trade in the South Indian Ocean, have described the cyclones of these districts as travelling (towards the W.N.W. or W.S.W.) at right angles, or nearly at right angles, to the course of the Trade-Winds, and have therefore insisted on the necessity of some cause for the progression quite independent of the prevailing wind ; and though no such cause has been suggested as accounting for this imaginary movement of tropical hurricanes athwart the Trade-Winds, it has been maintained that in our own latitudes the storms move from west to east entirely by reason of the feeble pressure of the warm and moist air on the east side of the revolution, where the wind is southerly, yielding to the superior pressure of the denser, colder, drier air on the west side, where the wind is northerly‡. But in every revolving

* See the barometric readings on board the ' Buzzard,' shown on the Admiralty Chart of the Nassau hurricane of 1866.

† Piddington, ' Sailor's Horn Book,' 4th edit. pp. 313-315.

‡ *Atlas des Tempêtes*, p. 21.

storm, in the tropics as well as outside, the wind on the eastern side is equatorial, is warmer and more humid than the polar air on the western side; and the amount of rain in a storm within the tropics is almost beyond comparison greater than that in a storm of higher latitudes. If the difference of temperature and the condensation on the eastern side of the area of least pressure could occasion the movement towards the east in temperate or high latitudes, much more could they do so in low. But Mr. Buchan conceives that if the progressive movement of a storm were due to the direction of a ruling wind, the velocity of the storm-wind would be greatly modified in the different quarters of the revolution—and that whilst (if the storm were moving towards the east) it would be very much increased on the south side of the centre of depression, it would, on the contrary, be very much diminished on the north side. To illustrate this, he supposes an extreme case of a storm in which the velocity of the wind is twenty-five miles an hour, and its rate of progression towards the east the same; and argues that there would be an absolute calm at any place on the northern side of the revolution*. I see no reason why in such a case there should not be such a calm; but it is not customary either amongst meteorologists, or among sailors, whom storms more personally affect, to speak of a wind of twenty-five miles an hour as a storm; but a whirl in which the wind has a velocity of fifty or sixty miles, and which is travelling eastwards at the rate of twenty or thirty miles an hour, may very well appear on its northern side as a wind of thirty or forty miles an hour, and on its southern side as a storm of seventy, eighty, or even ninety. The results of the observations which Professor Mohn has so carefully accumulated offer no contradiction to such a view, but, indeed, very strongly support it, as they lead him (although adhering to the purely meteorological theory) to estimate the frequency of *storms* in the southern and northern halves of these circles revolving round a centre of depression as in the ratio of 38 : 22, or very nearly 2 : 1†.

But if the storms are carried along in the body of the prevailing wind, they stand to it in exactly the relation that whirls of water do to the stream that sweeps them down; they exercise no more general influence on the wind than these do on the course of the river; and the onward flow of the one, as the downward flow of the other, is quite irrespective of these minor though frequently important disturbances.

The wide application of the very ingenious law proposed by Professor Buys-Ballot is apt to mislead those who trust too exclusively to meteorological, as opposed to geographical observa-

* Handy Book, p. 286.

† *Atlas des Tempêtes*, p. 12.

tions. In both hemispheres the normal west wind blows in accordance with this law; and the low barometer to the left or right, as the case may be, does not necessarily imply an independent rotatory motion of the air. In the same way the Trade-Winds of the Atlantic pass by the low pressure of the Doldrums according to the law, but without inclining towards or circling round it; and though it would seem highly probable that this low pressure adds strength to the Trades in some parts of the ocean, it evidently does not produce any tendency to that centripetal movement the theory of which is now so much in favour. As I believe that the whirls of air, when they do occur, are due mainly to geographical peculiarities, in which I would include the characteristics of the prevailing winds, so also I believe that the direction in which the whirl turns is guided entirely by the geographical conditions under which it is formed; but the discussion of this point is foreign to my present subject. It is, however, worth noticing that if, in any part of the world where the west winds blow, a whirl is formed which revolves the *wrong* way, the air, which in the first instance supplies it, is carried from a higher latitude to a lower, and must, at least sometimes, when thrown into an area of central calm, have both its temperature and humidity increased. I have shown how, on a very large scale, the system of winds on the equatorial side of each oceanic basin thus revolves the wrong way round a centre of high pressure; and it would seem possible that other centres of high pressure round which the winds occasionally circle, in what Mr. Galton has aptly designated "anticyclones," are formed in the same way. We cannot, however, lay much stress on this possibility; for since increase of temperature increases evaporation, and increased evaporation again lowers the temperature, the vagaries shown by the thermometer are frequently extremely puzzling; and the difficulty of establishing a satisfactory comparison between observations made in different places, at different hours, and at different heights above the sea-level has not yet been overcome. And it must be borne in mind that if the place of relative low pressure lies to the left, the place of relative high pressure must lie to the right; and the appearance of an anticyclone may be shown on the chart by the near approach of two, or occasionally even three centres of depression*.

The conclusion, then, to which this examination into the phenomena of cyclonic storms leads us is, that the centre of low pressure is formed, not by any meteorological agency, but by the circular rush of a body of air according to the known prin-

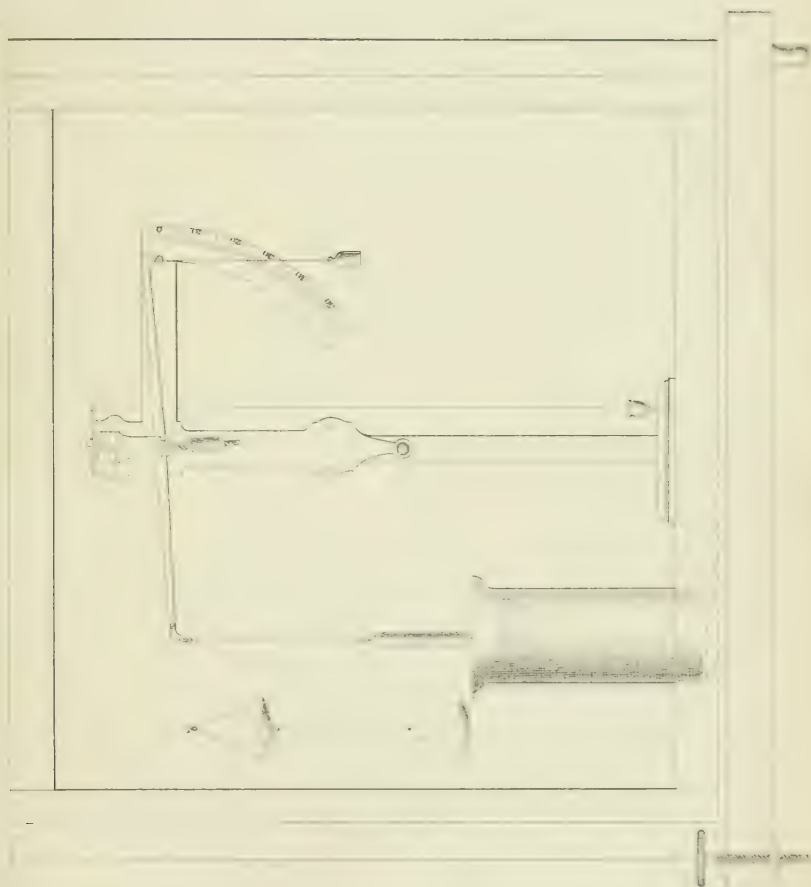
* For an example of this, see the Chart for March 28, 1868 (Morning), in the *Atlas des Tempêtes*.

ciples of fluids in motion, but that, when once formed, the ascending current, supplied by the vorticose movement due to the action of the centrifugal tendency in a confined space, gives rise to such excessive condensation that the depression of the barometer near the centre is very much increased, and most so within the tropics, where the absolute quantity and elastic force of vapour in the air are greatest. This increase of depression increases the force of the wind, which again increases the depression, the velocity of the ascending current, and the condensation. The various agencies thus continue to act and react on each other, so that it is impossible to separate the effects of each; but as the whirl is commenced and the centre of low pressure first formed by mechanical action, it is to it that I attribute the origin of the storm. Without the mechanical action such storms never occur, notwithstanding even excessive disturbances in the thermometric or hygrometric conditions of the atmosphere; and, on the other hand, gentler whirls and barometric disturbances within considerable limits frequently do occur without any corresponding manifestation of meteorological changes.

But this conclusion is, in its essential points, the same as that at which we previously arrived (namely, that differences of barometric pressure are in many instances caused by the winds), and justifies us in saying that the meteorological axiom, that all winds are caused by differences of barometric pressure, although presumably true on hydrostatic principles, is not supported, and is frequently contradicted, by positive evidence.

The prevailing winds of different localities, which thus, regularly or irregularly, produce many of the barometric variations, may be traced, more or less distinctly, to their geographical causes, as deflections of the great westerly wind which we find ruling at the surface of the earth in temperate latitudes, and which, from extreme north to extreme south, so far as our experience extends, is permanent in the upper regions of the atmosphere*. But the origin of this westerly wind is obscure. Recent observations seem to point towards the conclusion that the sun's atmosphere has a general movement from west to east, independent of, and additional to his proper movement of rotation, and almost exactly similar to that which, it appears to me, affects our own. If this is confirmed, it is at least a striking coincidence, and would seem to lend a very strong support to the various other reasons for believing that this dominant motion of our atmosphere is due to cosmical forces.

* I have treated of this question in detail in my 'Physical Geography;' to say more about it here would be merely to repeat the arguments and illustrations which I have there put forward.



LVII. *On the Tangent-balance, and its application to determining the Density of Solids and Liquids by direct reading.* By Professor K. W. ZENGER*.

[With a Plate.]

THE degree of delicacy of a balance depends on the weight of the arm m , the distance d of its centre of gravity from the axis of suspension, and finally on the length of the arm l . If p is a weight on one scale and u is the angle of deflection, then

$$\tan u = \frac{pl}{md}.$$

If the balance be so arranged that the constant

$$\frac{l}{md} = 1,$$

then

$$\tan u = p.$$

The tangent of the angle of deflection is equal to the weight p . If by means of a hook and a fine platinum wire there be suspended to one arm of the balance a small glass rod which dips in a liquid, for instance the specifically lightest sulphuric ether (of 0.736 spec. grav. at 0°), and if to the other end of the beam a small brass weight be hung and so arranged that the index stands at zero, we have as the equation for the angle of deflection,

$$\tan u_0 = (p - x + y_0) \frac{l}{md}, \quad . \quad . \quad . \quad . \quad (1)$$

where p is the weight of the brass weight, x the weight of the glass rod, y_0 its loss of weight in the ether.

As the angle will then be zero, it follows that

$$\begin{aligned} 0 &= p - x + y_0, \\ p &= x - y_0. \end{aligned}$$

Immersed in another liquid of greater specific gravity, the loss of weight y must be greater, and the index will give an angle u ; we shall then have

$$\tan u = (p - x + y) \frac{l}{md}, \quad . \quad . \quad . \quad . \quad (2)$$

The loss of weight may be supposed to consist of two parts—namely, the loss of weight in ether, plus the excess of loss of weight y_1 in the denser fluid—from which is obtained

$$\begin{aligned} y &= y_0 + y_1, \\ y_1 &= y - y_0. \end{aligned}$$

* Communicated by the Author.

We obtain from this

$$\tan u = (p - x + y_0 + y_1) \frac{l}{md},$$

and

$$\tan u = y_1 \frac{l}{md} = (y_0 - y) \frac{l}{md}.$$

Now if

$$\frac{l}{md} = 1,$$

then

$$\tan u = y - y_0.$$

But the losses of weight are the weights of equal volumes v of sulphuric ether and of a denser liquid; and therefore

$$\tan u = v(s - s_0),$$

in which s_0 and s are the specific gravities of the two liquids.

If the volume is equal to the unit of volume, we obtain

$$\tan u = s - s_0,$$

and finally

$$s = s_0 + \tan u;$$

for which, making the above supposition, and designating the densities by d_0 and d , we may write

$$d = d_0 + \tan u.$$

The tangent-balance (Pl. VI.) gives the density without using sets of weights, by merely reading the angle at the index of the balance, which plays on a circular limb 5 inches in length and divided into half degrees, the centre of which is in the axis of rotation of the beam.

The beam is provided with an adjustment, by means of which it may be raised and lowered so as to immerse the glass rod suitably.

The liquid is placed in a small beaker, which need not hold more than 2 or 3 cubic centims., so that a minimum of liquid can be used with entire certainty. The delicacy of the balance is such that a deflection of 1° indicates a weight of 17 milligrammes; and as the division extends to half degrees, and one tenth of a division may be estimated, it is possible to read to the twentieth of a degree; so that the balance indicates 0.8 milligram.

The density of a liquid, measured by the tangent-balance, is found by adding to the density of ether, or of any other liquid which corresponds to the zero of the division, the natural tangent of the angle of deflection which the index gives when the

glass rod is immersed in a denser liquid than the normal liquid (sulphuric ether).

If this liquid, for instance, is concentrated sulphuric acid, and the angle of deflection 48° , the density is

$$d = 0.736 + \tan 48^\circ = 0.736 + 1.1106 = 1.8466.$$

For solids a double pan is provided; the lower is immersed in water by depressing the beam; the upper one is loaded with pieces of the solid until the index stands at zero; thereupon the solid is placed upon the lower pan under water. If the angle of deflection of the unloaded beam is u , while u_1 is the angle which the index gives when the solid is on the lower pan under water, then the density

$$d = \frac{\tan u}{\tan u_1},$$

taking the density of water as unity.

Unloaded, the tangent-balance gives an angle $u = 19^\circ$; garnets are added until the index shows zero; and these are then placed on the pan which is under water, when the index gives the angle $u_1 = 10^\circ$; the density is therefore

$$d = \frac{\tan 19^\circ}{\tan 10^\circ} = \frac{0.3443}{0.1763},$$

for which we may write

$$\begin{aligned} d &= \tan 19^\circ \cot 10^\circ = 0.3443 \times 5.6713, \\ d &= 3.4697. \end{aligned}$$

The pyknometer gave $d = 3.470$.

For higher specific gravities this method becomes inaccurate; and therefore a load is placed upon one pan, which places the index at 45° to 50° when the lower pan is dipped in water; a piece of the body is laid upon the upper pan and the angle u noted; the piece is next brought under the liquid and the angle u_1 noted; if u_0 is the angle for the unloaded balance, we have for the density

$$D = \frac{\tan u_0 - \tan u_1}{\tan u_1 - \tan u}.$$

Thus, with an angle of 45° for the unloaded balance, three garnets upon the upper pan gave $u = 40^\circ 10'$, and upon the lower one $u_1 = 41^\circ 25'$; hence the density is

$$D = \frac{\tan 45^\circ - \tan 41^\circ 25'}{\tan 41^\circ 25' - \tan 40^\circ 10'} = \frac{1 - 0.882}{0.882 - 0.848},$$

$$D = \frac{0.118}{0.034} = 3.47.$$

The adjoined Table facilitates the operation for liquids; for it gives the natural tangent for the angle and also the density when the normal liquid is sulphuric ether of specific gravity 0.736 at 0°; a difference-table for $\frac{1}{2}^\circ$ or 3' facilitates interpolation.

TABLE of the Density of Liquids for $d_0=0.736$.

<i>u.</i>	<i>tan u.</i>	$\Delta 3'$	Density.	<i>u.</i>	<i>tan u.</i>	$\Delta 3'$	Density.	<i>u.</i>	<i>tan u.</i>	$\Delta 3'$	Density.
0° 30'	0.0087 _{8.8}		0.715	17° 30'	0.3153 _{9.6}		1.051	34° 30'	0.6873 _{12.9}		1.423
1 0	0.0173 _{8.7}		0.753	18 0	0.3249 _{9.7}		1.061	35 0	0.7002 _{13.0}		1.436
1 30	0.0262 _{8.7}		0.762	18 30	0.3340 _{9.7}		1.071	35 30	0.7131 _{13.2}		1.449
2 0	0.0349 _{8.8}		0.771	19 0	0.3443 _{9.8}		1.080	36 0	0.7265 _{13.5}		1.462
2 30	0.0437 _{8.8}		0.780	19 30	0.3451 _{9.9}		1.090	36 30	0.7400 _{13.6}		1.476
3 0	0.0523 _{8.7}		0.789	20 0	0.3640 _{9.9}		1.100	37 0	0.7536 _{13.7}		1.490
3 30	0.0612 _{8.7}		0.797	20 30	0.3739 _{10.0}		1.110	37 30	0.7673 _{14.0}		1.503
4 0	0.0699 _{8.8}		0.806	21 0	0.3839 _{10.0}		1.120	38 0	0.7813 _{14.1}		1.517
4 30	0.0787 _{8.8}		0.815	21 30	0.3939 _{10.1}		1.130	38 30	0.7954 _{14.4}		1.531
5 0	0.0873 _{8.8}		0.823	22 0	0.4040 _{10.2}		1.140	39 0	0.8098 _{14.5}		1.546
5 30	0.0963 _{8.8}		0.832	22 30	0.4142 _{10.3}		1.150	39 30	0.8243 _{14.8}		1.560
6 0	0.1051 _{8.8}		0.841	23 0	0.4245 _{10.3}		1.160	40 0	0.8391 _{15.0}		1.575
6 30	0.1139 _{8.9}		0.850	23 30	0.4348 _{10.4}		1.171	40 30	0.8541 _{15.2}		1.590
7 0	0.1228 _{8.9}		0.859	24 0	0.4452 _{10.5}		1.181	41 0	0.8693 _{15.4}		1.605
7 30	0.1317 _{8.8}		0.868	24 30	0.4557 _{10.6}		1.192	41 30	0.8847 _{15.7}		1.621
8 0	0.1403 _{9.0}		0.876	25 0	0.4663 _{10.7}		1.202	42 0	0.9004 _{15.9}		1.636
8 30	0.1493 _{9.0}		0.886	25 30	0.4770 _{10.7}		1.213	42 30	0.9163 _{16.2}		1.652
9 0	0.1584 _{8.9}		0.894	26 0	0.4877 _{10.8}		1.224	43 0	0.9325 _{16.5}		1.668
9 30	0.1673 _{8.9}		0.903	26 30	0.4986 _{10.9}		1.234	43 30	0.9490 _{16.7}		1.685
10 0	0.1764 _{9.0}		0.912	27 0	0.5093 _{11.1}		1.245	44 0	0.9652 _{17.0}		1.702
10 30	0.1853 _{9.1}		0.921	27 30	0.5206 _{11.1}		1.257	44 30	0.9827 _{17.3}		1.719
11 0	0.1944 _{9.1}		0.930	28 0	0.5317 _{11.3}		1.268	45 0	1.0000 _{17.6}		1.736
11 30	0.2033 _{9.1}		0.939	28 30	0.5430 _{11.3}		1.279	45 30	1.0176 _{17.9}		1.750
12 0	0.2126 _{9.1}		0.949	29 0	0.5543 _{11.5}		1.290	46 0	1.0355 _{18.2}		1.771
12 30	0.2217 _{9.2}		0.958	29 30	0.5658 _{11.6}		1.302	46 30	1.0538 _{18.5}		1.790
13 0	0.2309 _{9.2}		0.967	30 0	0.5774 _{11.6}		1.313	47 0	1.0724 _{18.9}		1.808
13 30	0.2401 _{9.2}		0.976	30 30	0.5890 _{11.6}		1.325	47 30	1.0913 _{19.3}		1.827
14 0	0.2493 _{9.3}		0.985	31 0	0.6009 _{11.9}		1.337	48 0	1.1106 _{19.7}		1.847
14 30	0.2586 _{9.3}		0.995	31 30	0.6128 _{12.1}		1.349	48 30	1.1303 _{20.1}		1.866
15 0	0.2679 _{9.4}		1.004	32 0	0.6249 _{12.2}		1.360	49 0	1.1504 _{20.4}		1.886
15 30	0.2773 _{9.4}		1.013	32 30	0.6371 _{12.3}		1.373	49 30	1.1708 _{21.6}		1.907
16 0	0.2867 _{9.5}		1.023	33 0	0.6494 _{12.5}		1.385	50 0			1.928
16 30	0.2962 _{9.5}		1.032	33 30	0.6619 _{12.6}		1.398				
17 0	0.3057 _{9.6}		1.042	34 0	0.6715 _{12.8}		1.410				

The above Table is inaccurate for greater densities than 1.2; this is obviated by a small additional weight which makes the index point to zero when the glass rod is immersed in water; the above Table is then to be used by adding unity to the tangent of the angle: the density is

$$d=1+\tan u.$$

The balance may also be used for determining the coefficient of expansion of liquids; the density needs only determining for two temperatures as different as possible.

Water at $18^{\circ} \cdot 9$ C. gave $u = 11^{\circ} 35'$, $d = 0 \cdot 9996$

„ $69^{\circ} \cdot 0$ „ $u_1 = 10^{\circ} 35'$, $d' = 0 \cdot 9799$

when the tangent-balance was adjusted to the density of absolute alcohol $0 \cdot 7946$.

From this are obtained the volumes

$$\begin{aligned} v &= 1 \cdot 000400 \\ v_1 &= 1 \cdot 002051 \\ \Delta v &= 0 \cdot 001651 \end{aligned}$$

for $\Delta t = 50^{\circ} \cdot 1$ C.

The correction for the expansion of the glass rod is $d\phi = 0 \cdot 00451$, and hence the actual expansion

$$\Delta v = 0 \cdot 002102.$$

According to Despretz, the volume of water at

$$\begin{aligned} 18^{\circ} \cdot 9 \text{ C. } v &= 1 \cdot 00156 \\ 69^{\circ} \cdot 0 \quad v_1 &= 1 \cdot 02200 \\ dv &= 0 \cdot 02044 \end{aligned}$$

Prague, December 17, 1870.

LVIII. *On the Scattering of Light by small Particles.* By the Hon. J. W. STRUTT, *Fellow of Trinity College, Cambridge**.

THE investigation of the diffraction of light by small particles, contained in the February Number of this Magazine, proceeds throughout on the assumption that the difference between two media which differ in refractive power is a difference of density and not a difference of rigidity. My object in the present communication is to attack the problem more generally, and to show that the more special hypothesis is in no degree arbitrary, but forced upon us by the phenomena themselves. The words "density," "rigidity" need not be interpreted literally, but are used in a generalized sense analogous to that given to "velocity" and "force" in the higher mechanics.

The first step is to find the equation of motion of an isotropic elastic medium whose density and rigidity may vary from point to point. If D denote the density and n the rigidity, a process similar to that used in Thomson and Tait's 'Natural Philosophy,' p. 530, leads us to the following:—

$$\begin{aligned} \frac{d}{dx}(m\delta) + \nabla_n \xi - D \frac{d^2 \xi}{dt^2} \\ - \frac{dn}{dx} \frac{d\eta}{dy} - \frac{dn}{dx} \frac{d\xi}{dz} + \frac{dn}{dy} \frac{d\eta}{dx} + \frac{dn}{dz} \frac{d\xi}{dx} = 0, \quad \dots \quad (1) \end{aligned}$$

* Communicated by the Author.

and two similar equations, where ξ, η, ζ are the displacements parallel to the coordinate axes,

$$\delta \equiv \frac{d\xi}{dx} + \frac{d\eta}{dy} + \frac{d\zeta}{dz},$$

$$\nabla_n \equiv \frac{d}{dx} \left(n \frac{d}{dx} \right) + \frac{d}{dy} \left(n \frac{d}{dy} \right) + \frac{d}{dz} \left(n \frac{d}{dz} \right).$$

If n and D were constant, equations (1) would be satisfied by

$$\left. \begin{aligned} \xi &= \eta = \delta = 0, \\ \zeta &= \zeta_0 = e^{ik(bt+x)}. \end{aligned} \right\} \dots \dots \dots (2)$$

In the application that we have to make, n and D may be supposed to be constant except within a small space T at the origin of coordinates, where they assume the values $n + \Delta n$, $D + \Delta D$. In consequence of this variation the equations of motion are no longer satisfied by (2); but we may take as the true values of the displacements, $\xi, \eta, \zeta_0 + \zeta, \delta$, where ξ, η, ζ, δ are small quantities of the order T , which are to be neglected when multiplied by $\Delta n, \Delta D^*$. Substituting in equations (1) and dropping the factor relating to the time, we get

$$\left. \begin{aligned} \frac{d}{dx} (m\delta) + \nabla_n \xi + Dk^2 b^2 \xi + \frac{dn}{dz} \frac{d\zeta_0}{dx} &= 0, \\ \frac{d}{dy} (m\delta) + \nabla_n \eta + Dk^2 b^2 \eta &= 0, \\ \frac{d}{dz} (m\delta) + \nabla_n \zeta + Dk^2 b^2 \zeta + \nabla_n \zeta_0 + Dk^2 b^2 \zeta_0 &= 0, \end{aligned} \right\} \quad (3)$$

and

$$k = \frac{2\pi}{\lambda}, \quad b^2 = \frac{n}{D},$$

or, since

$$\begin{aligned} \nabla_n \zeta_0 &= \nabla_{\Delta n} \zeta_0 + \nabla_n \zeta_0, \\ D\zeta_0 &= (D + \Delta D)\zeta_0 = D\zeta_0 + \Delta D \cdot \zeta_0, \end{aligned}$$

and

$$\begin{aligned} \nabla_n \zeta_0 + n k^2 \zeta_0 &= 0, \\ \left. \begin{aligned} \frac{1}{n} \frac{d}{dx} (m\delta) + \nabla \xi + k^2 \xi + \frac{1}{n} \frac{dn}{dz} \frac{d\zeta_0}{dx} &= 0, \\ \frac{1}{n} \frac{d}{dy} (m\delta) + \nabla \eta + k^2 \eta &= 0, \\ \frac{1}{n} \frac{d}{dz} (m\delta) + \nabla \zeta + k^2 \zeta + \frac{1}{n} \nabla_{\Delta n} \zeta_0 + \frac{\Delta D}{D} k^2 \zeta_0 &= 0. \end{aligned} \right\} \end{aligned}$$

Hence if $\varpi_3 = \frac{d\xi}{dy} - \frac{d\eta}{dx}$ &c. be the *rotations* of the medium,

* The effect of this is subsequently considered.

$$\left. \begin{aligned} \nabla \varpi_3 + k^2 \varpi_3 + \frac{1}{n} \frac{d}{dy} \left(\frac{dn}{dz} \frac{d\zeta_0}{dx} \right) &= 0, \\ \nabla \varpi_1 + k^2 \varpi_1 - \frac{d}{dy} \left\{ \frac{1}{n} \frac{d}{dx} \left(\Delta n \frac{d\zeta_0}{dx} \right) + \frac{\Delta D}{D} k^2 \zeta_0 \right\} &= 0, \\ \nabla \varpi_2 + k^2 \varpi_2 + \frac{d}{dx} \left\{ \frac{1}{n} \frac{d}{dx} \left(\Delta n \frac{d\zeta_0}{dx} \right) + \frac{\Delta D}{D} k^2 \zeta_0 \right\} \\ - \frac{1}{n} \frac{d}{dz} \left\{ \frac{dn}{dz} \frac{d\zeta_0}{dz} \right\} &= 0. \end{aligned} \right\} \quad (4)$$

Accordingly

$$\begin{aligned} \varpi_3 &= \frac{1}{4\pi n} \iiint \frac{\epsilon^{-ikr}}{r} \frac{d\zeta_0}{dx} \frac{d}{dy} \left(\frac{d\Delta n}{dz} \right) dv, \\ &= - \frac{1}{4\pi n} \iiint \frac{d\Delta n}{dz} \frac{d\zeta_0}{dx} \frac{d}{dy} \left(\frac{\epsilon^{-ikr}}{r} \right) dv, \\ &= - \frac{1}{4\pi n} \iiint \Delta n \frac{d\zeta_0}{dx} \frac{d^2}{dy dz} \left(\frac{\epsilon^{-ikr}}{r} \right) dv, \\ &= \frac{\Delta n T}{4\pi n} \frac{d\zeta_0}{dx} \cdot \frac{yz}{r^2} \frac{d^2}{dr^2} \left(\frac{\epsilon^{-ikr}}{r} \right), \end{aligned}$$

higher powers of $\frac{1}{r}$ being neglected. By similar reasoning,

$$\begin{aligned} \varpi_1 &= \frac{-T}{4\pi} \frac{\Delta D}{D} k^2 \zeta_0 \frac{y}{r} \frac{d}{dr} \left(\frac{\epsilon^{-ikr}}{r} \right) \\ &\quad - \frac{T}{4\pi} \frac{\Delta n}{n} \frac{d\zeta_0}{dx} \frac{xy}{r^2} \frac{d^2}{dr^2} \left(\frac{\epsilon^{-ikr}}{r} \right), \\ \varpi_2 &= \frac{T}{4\pi} \frac{\Delta D}{D} k^2 \zeta_0 \frac{x}{r} \frac{d}{dr} \left(\frac{\epsilon^{-ikr}}{r} \right) \\ &\quad + \frac{T}{4\pi} \frac{\Delta n}{n} \frac{d\zeta_0}{dx} \frac{x^2}{r^2} \frac{d^2}{dr^2} \left(\frac{\epsilon^{-ikr}}{r} \right) \\ &\quad - \frac{T}{4\pi} \frac{\Delta n}{n} \frac{d\zeta_0}{dx} \frac{z^2}{r^2} \frac{d^2}{dr^2} \left(\frac{\epsilon^{-ikr}}{r} \right); \end{aligned}$$

or, since

$$\zeta_0 = \epsilon^{ikx}, \quad \frac{d\zeta_0}{dx} = ik\epsilon^{ikx},$$

$$\left. \begin{aligned} \varpi_3 &= \frac{ik^3 T}{4\pi} \frac{\epsilon^{-ikr}}{r} \left(-\frac{\Delta n}{n} \frac{yz}{r^2} \right), \\ \varpi_1 &= \frac{ik^3 T}{4\pi} \frac{\epsilon^{-ikr}}{r} \left(\frac{\Delta D}{D} \frac{y}{r} + \frac{\Delta n}{n} \frac{xy}{r^2} \right), \\ \varpi_2 &= \frac{ik^3 T}{4\pi} \frac{\epsilon^{-ikr}}{r} \left(-\frac{\Delta D}{D} \frac{x}{r} + \frac{\Delta n}{n} \frac{z^2 - x^2}{r^2} \right). \end{aligned} \right\} \quad (5)$$

These are the component rotations. The resultant in the general case would be rather complicated, and is not wanted for our purpose. It is easily seen to be about an axis perpendicular to the scattered ray, inasmuch as

$$x\varpi_1 + y\varpi_2 + z\varpi_3 = 0.$$

Let us consider the particular case of a ray scattered normally to the incident light, so that $x=0$. Denoting for brevity the common factor by p , we have

$$\left. \begin{aligned} \varpi_3 &= -p \frac{\Delta n}{n} \frac{yz}{r^2}, \\ \varpi_1 &= p \frac{\Delta D}{D} \frac{y}{r}, \\ \varpi_2 &= p \frac{\Delta n}{n} \frac{z^2}{r^2}, \end{aligned} \right\} \dots \dots \dots (6)$$

whence

$$\varpi^2 = \varpi_1^2 + \varpi_2^2 + \varpi_3^2 = p^2 \left(\frac{\Delta n}{n} \right)^2 \frac{z^2}{r^2} + p^2 \left(\frac{\Delta D}{D} \right)^2 \frac{y^2}{r^2}.$$

Here we have reached a result of some importance and one which can be confronted with fact. For from the value of ϖ it appears that there is no direction in the plane perpendicular to an incident ray of polarized light in which the scattered light vanishes, if Δn and ΔD be both finite. Now experiment tells us plainly that there is such a direction, and therefore we may infer with certainty that either Δn or ΔD vanishes. So far we have a choice between two suppositions; either we may assume, as in my former paper, that there is no difference of *rigidity* between one medium and another, and that the vibrations of light are normal to the plane of polarization, or, on the other hand, that there is no difference of *density* between media, and then the vibrations must be supposed to be *in* the plane of polarization. The former view is the one adopted by Green and (virtually) by Cauchy in their theories of reflection; while the latter is that of MacCullagh and Neumann, which I now proceed to show is untenable.

Suppose then that $\Delta D=0$. Reverting to the general values of $\varpi_1, \varpi_2, \varpi_3$ in (5), we have

$$\left. \begin{aligned} \varpi_3 &= -p \frac{\Delta n}{n} \frac{yz}{r^2}, \\ \varpi_1 &= p \frac{\Delta n}{n} \frac{xy}{r^2}, \\ \varpi_2 &= p \frac{\Delta n}{n} \frac{z^2 - x^2}{r^2}; \end{aligned} \right\} \dots \dots \dots (7)$$

which show that there are in all six directions from 0 along which there is no scattered ray—two perpendicular to the plane (zx) of original vibration, and four in that plane inclined at angles of 45° to the original ray and its prolongation. No vanishing of the dispersed light in these oblique directions is known from experiment; but before unreservedly discarding the theory which indicates it, we ought to inquire how far our approximation is sufficient to warrant such a step. In neglecting the products of ξ , η , ζ with Δn , we have in reality omitted terms from the result which involve the square and higher powers of Δn , and it may be that the light corresponding to them would not vanish in the specified directions. I have not been able to satisfy myself whether this would be so or not; but I think that, in spite of ignorance on this point, the inference may be safely drawn that the theory is untenable; for the terms in question, depending on the square of the difference of rigidity, are proportional to $\frac{\Delta\mu^2}{\mu^2}$

(where μ is the refractive index), and become of less and less importance as the media approach one another in refrangibility. In the case of particles of mastic suspended in water, the indices are 1.5 and 1.33, and terms depending on the square of Δn must be comparatively small. Yet I could find no indication of a falling off of intensity in the predicted directions in some experiments that I made with precipitated mastic and soap, and accordingly conclude that the hypothesis of a constant density and variable rigidity must be rejected. The only alternative is to suppose, as in the February Number, that the æther preserves its statical properties unchanged when associated with matter, whose effect is therefore merely to increase the inertia of the vibrating parts in greater or less degree.

It may be worth notice that, according to the theory here combated, there would be two polarizing-angles, of $22\frac{1}{2}^\circ$ and $67\frac{1}{2}^\circ$ respectively, when light vibrating in the plane of incidence is reflected from the boundary of two media which differ but little in refrangibility, as may be seen from the reasoning of this paper by remembering that the square of Δn may be neglected. I need scarcely say that in such a case the polarizing angle is really 45° , and that the reflected light does not tend to vanish at the two first-mentioned incidences, whichever way the light may be polarized*.

In equations (5), putting $\Delta n=0$, we have

* There is a sense in which 45° is the first approximation to the polarizing-angle for all substances. The difference between the true value and 45° may be looked upon as a *correction* depending on the square and higher powers of the difference of optical density.

$$\left. \begin{aligned} \varpi_3 &= 0, \\ \varpi_1 &= p \frac{D' - D}{D} \frac{y}{r}, \\ \varpi_2 &= -p \frac{D' - D}{D} \frac{x}{r}, \end{aligned} \right\} \dots \dots \dots (8)$$

which correspond to the results already obtained. Since the optical density is proportional to the square of the refractive index,

$$\frac{D' - D}{D} = \frac{\mu'^2 - \mu^2}{\mu^2} \dots \dots \dots (9)$$

In a note to my previous paper I mentioned that no change is required in (8), even though the terms containing the square and higher powers of $\frac{D' - D}{D}$ are retained. As I there showed,

the density of a medium may always be supposed to be changed, even in the most arbitrary manner, if suitable bodily forces proportional to the variation of density and to the actual acceleration are conceived to act upon it, while the motion remains absolutely the same as before. The waves thrown off from a small particle which lies in the path of a beam of light are those due to a set of forces proportional to $D' - D$, and parallel to the actual vibrations acting through the space T occupied by the particle. In calculating the effect of the forces, the variation of density is to be taken into account, unless we are content to neglect the square of $D' - D$. But by a second application of our principle we see that the density within the space T may be supposed to be D instead of D' , provided we introduce a second set of forces proportional to $D' - D$ and to the acceleration at T . Now it may be proved (Thomson and Tait, p. 569) that the effect of a bodily force applied through a small space T to an elastic medium diminishes without limit with T *even within the region of application*. Accordingly the acceleration at T caused by our first set of forces is of a higher order of magnitude than the forces themselves, and thus, whether $D' - D$ be small or not, the effect of the second set is to be neglected. The error caused by taking in the calculation of the first set the undisturbed instead of the actual acceleration is evidently smaller still.

If it were desired to continue the approximation, some further supposition would be necessary as to the *shape* of the disturbing particles. The leading term, we have seen, depends only on the volume; but the same would not be true for those that follow. However, little exception could be taken to the assumption of a spherical form; and in that case there is no difficulty in proceed-

ing further; but I have not arrived at any results of interest. Without calculation, we may anticipate that, as the diameter of the particles approaches in magnitude the quarter wave-length, the amplitude of the diffracted vibration will begin to increase less rapidly than T , and that about the time the half wave-length is passed an absolute diminution will set in. Of course, when the incident light is compound, the more refrangible elements will be the first to show a sensible deviation from the more simple law.

In his interesting experiments with precipitated vapours, Professor Tyndall* found that when the particles of the cloud illuminated by unpolarized light from the electric lamp had attained such a size that the light discharged normally had lost most of its power of affecting the naked eye with the sensation of colour, even then by analyzing the light with a Nicol placed in its position of minimum transmission the azure could be revived in increased splendour. Professor Tyndall calls this the "residual blue." Experimentally it is doubtless more convenient to analyze the light after diffraction from the cloud; but in theoretical explanation and deduction it is simpler, and comes to the same thing in the end, to consider the original beam as polarized before it falls on the cloud. The residual blue is then the light discharged from the cloud in a direction parallel to that in which the incident light swings. The complete explanation of this and other allied phenomena is yet to be made out; but one thing we learn from our theory, if indeed it is at all to be depended on. However large the particles may be, the light scattered or reflected† parallel to the primary vibrations depends on the square and higher powers of $\frac{D' - D}{D}$, or, in experimental

language, of $\frac{\mu'^2 - \mu^2}{\mu^2}$. It is easy to see, too, that the first term in the expression of the amplitude must contain a much higher inverse power of λ than λ^{-2} , and that if it stood alone it would correspond to a compound light of a much richer colour than that due to very small particles acting in the ordinary way. Still I cannot honestly say that the residual blue is predicted by theory: before the light discharged in this unfavourable direction could become at all sensible, the particles must have grown to such a size that their diameter would bear no inconsiderable proportion to the waves of light; and then we have no right to suppose that the first term in the expansion proceeding by powers of the diameter may be taken

* Phil. Mag. vol. xxxviii. p. 156. Phil. Trans. 1870.

† This may be verified with Fresnel's expression for the intensity of the light regularly reflected when the plane of polarization and plane of incidence include a right angle.

as representing with sufficient approximation the entire series. Indeed the residual blue appears to be rather capricious in its appearance, and to depend on conditions not yet fully known. I may mention that I have not been able to detect any unusually intense coloration in that part of the light from the sky which vibrates in a plane passing through the sun. This is the more remarkable, because it might be supposed that a part at least is light which has twice undergone diffraction, in which case the intensity would vary as λ^{-8} if otherwise undisturbed. But we must not forget that, of the indirect light illuminating the higher strata of our atmosphere, a very considerable fraction must come from the earth itself; and this certainly is coloured any thing but blue. It would be interesting to observe whether the residual light from parts of the sky 90° distant from the sun is in any way dependent on the character of the earth's surface—whether, for example, it is the same as usual over water or when the ground is covered with snow. I presume that with the precipitated clouds there is no question of light diffracted more than once.

Theory would lead us to anticipate that the optical density of the particles of foreign matter may have a large influence on the development of the residual blue. If the particles and the medium in which they are suspended have nearly the same refrangibility, the light emitted parallel to the original vibrations may be expected to be very feeble, not only absolutely, but in comparison with that emitted in other directions. Professor Tyndall's method of precipitating organic vapours (some of which may have a high optical density) in air is then more favourable than the suspension of mastic or other moderately dense solids in water, as used by Brücke and other physicists.

I take this opportunity of referring to the observations of Roscoe on the photographic power of skylight, with which I have only lately become acquainted. The comparison of photographic with luminous intensities is well adapted to exhibit differences of quality related in a simple manner to the wave-length. The very small chemical action of the direct solar rays, as compared with what might have been expected from their intense action on the retina, is a striking verification of the theoretical results developed in the February Number of this Magazine.

LIX. *On the Capillary Phenomena of the common Surface of two Liquids.* By G. QUINCKE.

[Continued from p. 390.]

V. *On the spreading of liquids on other liquids.*

24. **I**F three common surfaces of three liquids intersect in a (curved) line, then three forces operate on a particle of the substance, P, in the line of intersection, which lie in the per-

pendicular plane of the element P of the curve. These forces are equal to the capillary constants or tensions of the surfaces of the three capillary surfaces, and in equilibrium when the equation

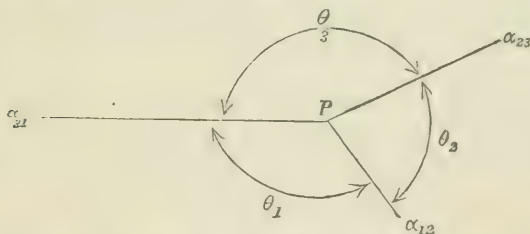
$$\frac{\alpha_{12}}{\sin \theta_3} = \frac{\alpha_{31}}{\sin \theta_2} = \frac{\alpha_{23}}{\sin \theta_1} \quad \dots \quad (1)$$

is satisfied.

In this equation $\theta_3, \theta_2, \theta_1$ denote the angles which the elements of the meridian of the curved capillary surface intersecting at the point P, whose directions coincide with those of the forces α_{12}, α_{23} , and α_{31} , make with one another. α_{12} is the capillary constant of the common surface of liquids 1 and 2, &c.

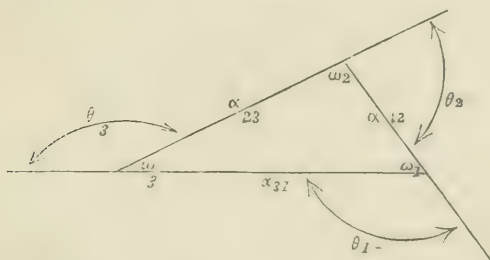
A lens-shaped drop of water on a surface of mercury, over which is air or oil, would correspond to the given hypotheses. The mercury may represent liquid 1, the water liquid 2, and the air or oil liquid 3. $\theta_3, \theta_2, \theta_1$ are the angles at the common edge of the wedge-shaped pieces of the three liquids, as may be seen from fig. 1.

Fig. 1.



The angles $\theta_3, \theta_2, \theta_1$ may be replaced by their supplements $\omega_3, \omega_2, \omega_1$ in equation (1); these are then the angles of a triangle whose three sides are α_{12}, α_{31} , and α_{23} respectively, or whose sides are parallel to the three meridian-elements of fig. 1, as fig. 2 shows.

Fig. 2.



If therefore a triangle be constructed whose three sides are proportional to the capillary constants (tensions of the surfaces) of the

common surfaces of three liquids meeting in a point, then the exterior angles of this triangle give the angles at the edge of the said three liquids for this point*.

It follows directly from this, that, along the curved line which cuts the three liquids, the marginal angle within each liquid must be constant, since the magnitudes α only depend on the nature of the liquids in contact, and are constant within the same liquid surface.

The sectional curve of the three surfaces must be a circle. In fact experiment at once shows the correctness of these last two conclusions when water is placed on the ordinary horizontal surface of mercury, where the water forms upon the mercury a lens-shaped drop or a thin layer with a circular opening (compare § 27).

If of the capillary constants α_{12} , α_{31} , α_{23} , and the angles θ_3 , θ_2 , θ_1 , three magnitudes are known, from these the remaining three can be found, and by the same methods which serve to determine the sides and angles of a triangle from three given elements.

For the case in which two liquids only are brought into contact (for example, water placed on mercury in air), $\alpha_{31} = \alpha_1$, $\alpha_{23} = \alpha_2$, or α_{31} and α_{23} denote the capillary constants of the free surface of liquids 1 and 2.

25. With the magnitudes of the capillary constants determined by means of experiment in the first section of this memoir, the angle of the edge can be calculated from the equation

$$\begin{aligned}\alpha_{12}^2 &= \alpha_{31}^2 + \alpha_{23}^2 - 2\alpha_{31}\alpha_{23} \cos \omega_3 - \cos \theta_3 \\ &= \cos \omega_3 = \frac{\alpha_{31}^2 + \alpha_{23}^2 - \alpha_{12}^2}{2\alpha_{31}\alpha_{23}}. \quad . \quad . \quad . \quad . \quad (2)\end{aligned}$$

Equations analogous to the above can be established for ω_2 and ω_1 .

The angle θ_3 is impossible (that is, a spreading of liquid 2 over the common surface of liquids 1 and 3 takes place) when $\cos \omega_3 \geq 1$,

$$\begin{aligned}\alpha_{31}^2 + \alpha_{23}^2 - \alpha_{12}^2 &\geq 2\alpha_{31}\alpha_{23}, \\ (\alpha_{31} - \alpha_{23})^2 &\geq \alpha_{12}^2, \\ \pm (\alpha_{31} - \alpha_{23}) &\geq \alpha_{12}. \quad . \quad . \quad . \quad . \quad (3)\end{aligned}$$

A liquid 2 placed on the surface of a liquid 1 which is bounded by air, spreads itself out, provided

$$\alpha_{12} \leq (\alpha_1 - \alpha_2); \quad . \quad . \quad . \quad . \quad . \quad (4)$$

that is, when the capillary constant of the common surface of liquids

* This proposition was, I believe, first enunciated by Neumann.

1 and 2 is equal to or less than the difference of the capillary constants of the free surface of liquids 1 and 2, or than the value of the capillary constant of the common surface of two liquids deduced from Poisson's theory.

Moreover this proposition follows, when the question is not to determine the angle ω_3 from it, simply from the observation that the difference of any two sides of a triangle must always be less than the third side.

For liquid 1 the liquid with the greater capillary constant must always be chosen, as the particles will adhere whose reciprocal attraction is the greater, or the particles of that liquid which possesses the greater (dependent on the reciprocal attraction) capillary constant.

The sign of equation (3) is always to be so chosen that the left side will be a positive magnitude, as a capillary constant or surface-tension of a liquid is an essentially positive magnitude.

It may here be mentioned that the whole theory of capillarity may be deduced from the principle that the surface of a liquid is a minimum. It appears that by bringing several liquids together they so arrange themselves that the sum of their surface-tensions becomes the least possible.

If a liquid n be displaced from the common surface of liquids 1 and n by a liquid 2, then the liquid 2 can be displaced by a liquid 3 from the common surface of liquids 2 and n , and so on. The conditions, according to equation (3), are

$$\alpha_{1n} > \alpha_{12} + \alpha_{2n},$$

$$\alpha_{2n} > \alpha_{23} + \alpha_{3n},$$

or (by adding these inequalities)

$$\alpha_{1n} > \alpha_{12} + \alpha_{23} + \dots \alpha_{n-1n} \dots \dots \dots (5)$$

For a gas or for rarefied air as liquid n , the suffix n would be omitted in the above equation, and α_1 and α_{n-1} would denote the capillary constant of the open surface of liquids 1 and $n-1$.

The different common surfaces then succeed one another as if they were arranged according to the magnitude of their capillary constant or tension of surface. The thickness of the single layers of liquids can never be 0, and must, in case the magnitudes α have the usual signification, be $>2l$, or greater than twice the radius of the sphere of action.

If the thickness of liquids 2, 3, $\dots n-1$ is very small, as, for example, in the experiments of Section II., then the sum of the single tensions of surfaces is equal to the collective tension of the superposed surfaces, and always less than the tension of the original (open or bounded by liquid n) surface of liquid 1.

26. The best method of observing the spreading of a liquid 2

on the surface of another liquid 1 consists in noticing in the drop or bubble the change of form which accompanies the spreading.

According to what has been said in § 25, the flat drop must in spreading become flatter, or, to express it more accurately, the magnitude $K - k$ must diminish. The entire height of the drop K will likewise diminish as soon as the angle θ does not materially alter.

In fact the experiments described in Section II. (§§ 11 to 14) show this decrease.

It follows from the summary given in § 10, Table X., that Poisson's formula does not hold for mercury or water and a series of liquids. It is thus in agreement with theory, and could have been foretold by it, that bisulphide of carbon, olive-oil, oil of turpentine, and petroleum spread themselves on flat air-bubbles in water, as I have proved in § 12, or that water, olive-oil, and oil of turpentine spread themselves on flat drops of mercury in air, as I have proved in § 13.

Absolute alcohol likewise spreads on a flat air-bubble in olive-oil (§ 12), since also for alcohol and olive-oil the capillary constant of the common surface, 0.226 milligrm., $= \alpha_{12}$, is $< \alpha_1 - \alpha_2$, which $= 3.760$ milligrms. $- 2.599$ milligrms. $= 1.61$ milligrm.

The smallest value of the capillary constant α_{12} is 0. In this case the two liquids are miscible in all proportions, and equation (3) always holds good; the liquid 2 with smaller capillary constant α_{23} spreads on liquid 1 with greater capillary constant α_{13} .

In fact it has been shown in § 14 that oil of turpentine spreads on the surface of flat drops of olive-oil in water, or on flat drops of bisulphide of carbon in water.

Flat drops of water in olive-oil retain their form if permitted to rest on a layer of mercury over which the olive-oil is poured. $K - k$ is the same for these drops as in flat bubbles of olive-oil in water (§ 7); it becomes less as soon as alcohol is brought to the boundary of oil and water—a proof that the alcohol spreads itself out on this bounding surface, as might have been foretold from the numbers of Table X. § 10 by help of the theory.

In like manner olive-oil, oil of turpentine, and petroleum (liquid 2), as was shown in § 14, spread themselves out on flat drops of mercury (liquid 1) in water (liquid 3), as α_{12} , for these liquids (according to Table X. § 10) 34.19 milligrms., 25.54 milligrms., and 28.94 milligrms. respectively, is less than $\alpha_{13} - \alpha_{23}$; that is, less than 40.48 milligrms., 41.40 milligrms., and 38.75 milligrms.

This spreading may also be observed in fused substances.

In the case of fused metals, when they alloy with each other in all proportions, α_{12} also equals 0.

If on a flat drop of gold or silver melted on charcoal, of which

the capillary constants* are 131.5 and 79.75 milligrms. respectively, a trace of lead† be placed, whose capillary constant is 45.66 milligrms. at 330°, and at 1200° or 1000° is consequently considerably less, the height of the drop and $K-k$ are immediately seen to decrease considerably, as theory requires. The melted lead (with smaller capillary constant) at once coats the whole drop, and the tension of the surface becomes considerably less than before.

The magnitude $K-k$, which for pure gold and silver amounts to 4 millims., decreases here 1 millim. or more. There is possibly superficial oxidation of the lead coating, although the surface of the drops retains its metallic appearance.

Flat drops of copper covered with a very thin layer of oxide are much shallower than those with a pure surface, because the oxide of copper which covers the surface has a much smaller capillary constant than copper. In this case α_{12} is assumed = 0 for the limit of copper and oxide of copper, since, from Matthiessen's experiments‡, the oxide appears to dissolve in the metal.

The layer which induces this change in the form of the drop is so remarkably thin that it is completely beyond all other methods of observation, spectral analysis perhaps excepted§.

If a substance when placed on a liquid is very quickly dissolved by the latter, it is easy to understand that the change in shape of the capillary surface may not occur. This is the case, for example, when oil of turpentine is placed on flat drops of bisulphide of carbon in water (compare § 14).

Small quantities of tin, copper, or silver placed on flat drops of melted gold, or tin on flat drops of melted silver, do not materially change the form of these drops.

Thus these metals appear to be dissolved more quickly than lead by melted gold and silver. I may perhaps have even overlooked a trifling change of form, as I was obliged to make this experiment on comparatively small drops from scarcity of pure substance.

27. From the magnitude of the capillary constants of the common surface of mercury and the liquids adduced in the first section (Table X. § 10), it follows that α_{12} is always less than $\alpha_1 - \alpha_2$, that consequently, according to equation (3) § 25, all the liquids named in that Table, especially water and an aqueous solution of hyposulphite of soda, must spread themselves out on a free surface of mercury.

In apparent contradiction to this is the fact that lenticular

* Pogg. *Ann.* vol. cxxxviii. p. 148 (1869).

† Ibid. vol. cxxxv. p. 642 (1869).

‡ Ibid. vol. ex. p. 224 (1860).

§ Compare Pogg. *Ann.* vol. cxxxviii. p. 147 (1869).

drops of water can be placed on a surface of mercury, and that they, as Draper* and Paalzow† have shown, contract and assume smaller diameters if an electric current is caused to enter the drop of water and pass out through the mercury. The lenticular drops, on the contrary, spread themselves out when the electric current flows in the opposite direction. Even very weak electric currents I have found produce this effect. The contraction of the drops increases with the duration and strength of the electric current. It is seen from this that the nature of the bounding surface has a real influence on the shape of the drop of water, that traces of a third substance brought to the separating surface of mercury and water (as the small quantities of hydrogen and oxygen liberated here by electrolysis) influence the form of the drop, even if from their small quantity they can scarcely be perceived by other methods of observation.

Paalzow has shown that contraction of the drop of water can be produced by the addition of hyposulphite of soda to the water, and by the addition of chromic acid an expansion of the drop. He assumes that even the purest mercury contains oxygen and a small quantity of oxide on its surface; by means of hydrogen electrolytically separated, or the addition of hyposulphite of soda, this layer of oxide is reduced, the capillary relation of the mercury to water is altered, the repulsion between the two liquids is increased, and thus the contraction of the drop is brought about.

If this explanation were correct, it would follow that the mercury examined in § 8 must have been coated with a layer of oxide, and the numbers in Table X. § 10 for mercury and water must refer to mercury coated with a layer of oxide, for mercury and hyposulphite of soda to mercury with a pure surface; and it would still be unexplained why drops of water and of hyposulphite of soda do not spread on drops of mercury.

But the contradiction immediately disappears if pure mercury be applied, not containing a trace of a foreign substance, particularly of a fatty or etherial oil. This is obtained in the following manner.

The mercury is digested for a long time under frequent agitation with concentrated sulphuric acid, to which a couple of drops of nitric acid are added, in order to free it from an admixture of oxide and foreign metals. It is afterwards separated from the sulphuric acid and the insoluble salts by being run repeatedly through a funnel made of clean writing-paper whose aperture is not too small, and which is stuck together on the outside with

* Phil. Mag. vol. xxvi. p. 187 (1845). In this memoir, however, the direction of the current is reversed.

† Pogg. Ann. vol. civ. p. 420 (1858).

sealing-wax. The apparently pure mercury is then shaken for a long time with distilled water, or (and this is a better process) it is allowed to run again and again through a paper funnel with a very narrow opening into a porcelain dish filled with hot distilled water. The foreign liquids, acids, oils, &c. mixed with the mercury dissolve in the water. The water is separated from the mercury, as the sulphuric acid was, by means of a paper funnel, in which a funnel made of blotting-paper, with a larger opening, can be placed. In order to remove the last trace of water, the mercury is finally passed through a funnel made of writing-paper with a narrow opening into a warmed porcelain dish*, and in the same manner into a dry flask, in which, after the latter has been corked, it is allowed to cool.

If the mercury thus purified be placed in a watch-glass which has been washed with alcohol, dried with a clean cloth, and warmed over a gas-jet, then water and an aqueous solution of hyposulphite of soda will spread themselves out upon it.

If the mercury be allowed to run through a funnel of writing-paper with a narrow opening into a warmed clean flask and then cooled in the well-corked flask, water will then spread on the surface of the cold mercury.

If the surface of the mercury be touched with a freshly drawn-out glass thread 0.1 millim. or smaller in diameter, on the surface of which there is a trace of olive-oil or oil of turpentine, water will contract upon it into a lens-shaped drop; at the same time the drop of water changes its position on the surface of the mercury, being driven from the place on which the oil was put.

The diameter of the drop of water diminishes as the quantity of oil placed on the surface of the mercury is increased. The superposed layer of oil spreads itself on the surface of the mercury to a thin pellicle of uniform thickness, lessens, as was explained in § 13, the surface-tension of the mercury, and equation (2) § 25 gives a real value of angle ω_3 . The diameter of the drop of water diminishes as the thickness of the layer of oil on the surface of mercury increases. If the layer of oil spreads itself out on one side of the drop of water and diminishes the surface-tension of the mercury in that place, then the whole drop will

* The porcelain dish shows in the place where the stream of mercury fell a system of Newton's coloured rings of an irregular elliptical form. The coloured layer, whose thickness decreases more slowly towards the outside than towards the inside, exhibited in the thickest place, after from 10 to 15 pounds of mercury had run out, blue and violet of the first order to yellowish green of the second order in reflected light; it was difficultly soluble in ClH , but easily so in $\text{NO}^3 \text{H}^2$, and I am inclined to think it was oxide of mercury. Whether the electricity excited by the friction of mercury and porcelain contributed to its formation, further experiments must teach.

be drawn over to the other side, where the unchanged surface of the mercury has the greater tension.

If hyposulphite of soda be placed on the lens-shaped drop of water, it contracts still more, since the capillary constant of the common surface of mercury and hyposulphite of soda is greater than the capillary constant of the common surface of mercury and water.

If the glass thread covered with oil is brought into contact with the free surface of a lens-shaped drop of water on mercury, the oil spreads on the water, diminishes the tension of its surface, and the diameter of the drop increases. If at the same time the glass thread wetted with oil be also, through immersion in the water, brought into contact with the common bounding surface of mercury and water, the oil spreads itself out upon it, diminishes its tension (§ 14), and the drop of water assumes a still greater diameter.

If the quantity of oil placed on the free surface of the water or on the surface bounded by the mercury is too great, the little drops of oil slide on the surface covered with oil (compare § 29) towards the periphery of the lens-shaped drop, spreads on the free surface of the mercury, and with the diminished tension of the latter the drop is observed to contract.

It is well to remark that small quantities of oil placed on the surface of a liquid x do not overstep the sharp edge of the lens-shaped drop of water. The edge acts as the sharp-cut edge of a vertical tube, in which the magnitude of the falling drop is independent of the substance of the tube, or of the angle which the liquid forms with the substance of the tube.

By placing a greater quantity of oil on the free surface of the water or of the mercury, the lens-shaped drop does not form a sharp edge, the oil spreads itself over the entire surface, whilst a skin of oil forces itself between the common surface of water and mercury, and there arises a lens-shaped drop of water which is arched strongly above and but slightly below. This form permits the theory to be foreseen from the numbers of Table X. § 10, as the cooperating tensions of the surfaces in the periphery of the lens are:—

$$\alpha_{13} = 34.19 + 3.76 = 37.95 \text{ milligrms.},$$

$$\alpha_{12} = 34.19 + 2.10 = 36.29, \quad ,,$$

$$\alpha_{23} = 2.10 + 3.76 = 5.86. \quad ,,$$

The upper portion of the liquid lens is nearly a hemisphere.

In air mercury condenses the vapour contained in the atmosphere so rapidly, that only under particularly favourable circumstances does a spreading of water on the free surface of the mercury take place. Generally the water remains as a lens-

shaped drop whose diameter quickly diminishes, since the tension of the surface of the mercury is more quickly changed than that of the water, in consequence of this condensation. The angle between water and mercury changes with the quantity and nature of the thin layer of liquid which has formed itself out of the condensed moisture and has coated the mercury. This also explains the different form of the lens-shaped drop of water which is generally observed on the mercury, unless certain precautionary measures have been taken.

A flat lens-shaped water-drop frequently contracts on the (impure) surface of mercury when the latter is breathed on. If air be passed through Babo's pearl-tubes wetted with water, this effect is considerably weakened; hence it proceeds from the foreign substances in the moisture of the air breathed. After a short time they disappear, and the drop of water again assumes its original form.

I believe that the layer of a liquid, the thickness of which does not exceed one-millionth of a millimetre, can be perceived in this manner, and that this method under favourable circumstances even surpasses in sensitiveness the one depending on optical phenomena.

Drops of an aqueous solution of hyposulphite of soda always assumed the shape of a lens on the cold surface of mercury in the experiments which I made. It must remain doubtful whether the non-appearance of the spreading is to be ascribed to the impurity of the mercury or to small errors in the numbers of Table X. § 10. Owing to the great difficulty of these researches, I cannot assert that perfectly pure mercury was used; and the question can only be decided by an expenditure of time and means which, I regret to say, I cannot at present command. The impurities of the mercury, in case such were actually present, cannot have had a material influence on the remaining experiments of this communication or the conclusions derived from them.

If the common boundary of mercury and water be covered with electrolytically separated gaseous hydrogen, the capillary tension of this limiting surface will approach the sum of the capillary tensions of a free surface of mercury and a free surface of water (that is, the magnitudes $55.03 + 8.25 = 63.28$ milligrms.), and will be so much greater than the capillary tension 42.58 of the common contact-surface of water and mercury the greater the quantity of hydrogen liberated on this surface. Hence the contraction of the drop of water must increase with the quantity of hydrogen liberated, as experiment also shows.

The layer of oxide formed by means of the liberated oxygen on the mercury surface diminishes the capillary tension of the

common surface of mercury and water, and the diameter of the drop increases.

The motion of the particle of liquid on the surface of mercury is considerably diminished by a thin layer of liquid which covers the surface of the mercury.

Lens-shaped drops of water and hyposulphite of soda with angles of different magnitudes can swim simultaneously on a surface (impure) of mercury.

The thin layer of liquid with which an apparently pure surface of mercury is covered may differ in thickness and in nature in different places. The lens-shaped drops of water have then different angles on the different portions of the mercury surface.

If by breathing on the surface of the mercury many such lens-shaped drops of equal magnitude are produced, then the places with equal and those with different angles are easily recognized. A so-called Moserian figure (*Hauchbild*) is seen. To the Moserian figures produced in a similar manner on solid substances, such as glass or metal plates, amongst which are to be included the images (Daguerreotypes) generated on insolated plates of iodide of silver by drops of mercury, I shall recur on another occasion.

Petroleum, alcohol, and ether produce phenomena similar to fatty and volatile oils. Instead of placing the easily vaporized liquids directly on the surface of mercury, it is frequently sufficient to vaporize a drop of the liquid in question in the neighbourhood of the surface of mercury and to allow the vapour to be condensed, which then spreads itself out on the surface of the mercury as a coherent layer, and gives rise to the movements or changes in form of the lens-shaped drops of water on the mercury in the manner described.

The effect of the thin layers of liquid on the surface of water and of mercury reaches a maximum when the thickness exceeds a magnitude $2l$,—that is, twice the distance at which the molecular forces of capillarity are still effective. By observing the maximum effect which a measured quantity of liquid produces on a surface of mercury of known magnitude, the magnitude $2l$ can therefore be determined. I shall take an opportunity of returning to this.

In the experiments described in § 8 on drops of mercury in different liquids, a trifling impurity in the mercury has but little disturbing influence, because the surrounding liquid 2 (for example, the water in which the drop of mercury lies) has a greater volume, and removes the impurity by dissolving it. The determination of the capillary constant α_{12} refers, therefore, to the surface of contact of, at all events, approximately pure mercury and water.

Instead of forming a lens-shaped drop, the water may also cover the mercury as a thin layer with a circular opening. The angle of the water with the surface of the mercury is the same in both cases.

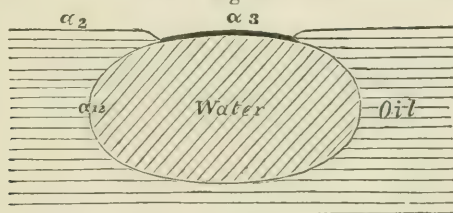
28. If a drop of liquid 1 be placed on a liquid 2 so that

$$\alpha_1 > \alpha_2, \quad \alpha_{12} < \alpha_1 - \alpha_2,$$

then liquid 2 will spread over the whole drop of liquid 1, and this will assume a lens-shaped form.

If a drop of water be placed on olive-oil, then its upper surface will be coated with a thin layer of oil, so that a small drop of oil, as Lüdtege* has shown, will spread on it as on a free surface of oil. The greatest diameter of the drop of water considerably exceeds the diameter of the circular line of intersection of the upper surface of the lens and of the free surface of the oil, which in this place is conically depressed. The drop has something of the form shown in fig. 3. This is in perfect agreement with

Fig. 3.



theory, since on an element of the circular line of intersection the following tensions act (according to Table X. § 10),

$$\alpha_2 = 3.76, \quad \alpha_{12} = 2.10 \text{ milligrms.},$$

$$\alpha_3 = 3.76 + 2.10 = 5.86 \quad ,,$$

in which α_3 denotes the tension of the surface of the oil-layer on the upper surface of the lens.

The diameter of the circular line of intersection increases or decreases when α_3 is diminished by placing oil of turpentine on the upper surface of the lens, or when α_2 is diminished by placing the same liquid on the free surface of the oil. The increase or decrease, however, is far from being so striking as in the analogous phenomena with lens-shaped drops of water on mercury.

29. Just as the vapours of foreign liquids contained in the atmosphere condense and spread over free surfaces of mercury, so also do they on the free surfaces of other liquids, *e. g.* water. If these impurities are not dissolved by the condensing liquid 1 with great facility, they form a thin coating, which may exer-

* Pogg. Ann. vol. cxxxvii. p. 372.

cise some influence on the spreading-out of other liquids, 2, on the surface of liquid 1.

The Moserian images on different liquid surfaces are also explained by the different angles which the condensed water-drops make with the surface of liquid 1, rendered impure from various causes.

Of the various cases of spreading of a liquid 2 on a liquid 1, the spreading of fatty oils on water especially has been frequently investigated, as is shown by the list given below*, although it is by no means complete.

When a small quantity of fatty oil (for example, a drop of olive-oil hanging to the end of a glass rod) is placed on the free plane surface of water or mercury in a large vessel, it spreads itself out as if it were on a curved surface.

If the quantity of oil is not too small, so that it is not quickly dissolved by the liquid below, then the film of oil after a time contracts to lens-shaped drops. In a similar manner a second drop of oil remains on the surface of water or mercury, upon which the first drop has already spread itself out.

If a surface of water has stood a long time in the open air so that its open surface is rendered sufficiently impure by a condensed layer of liquid condensed from the atmosphere, then the drops of oil first placed on it assume the lens shape, as G. Hagen was the first to observe.

The explanation of these phenomena must, in my opinion, be sought in the change which the oil undergoes in contact with water or mercury, either through solution or a chemical combination (perhaps under the influence of atmospheric air). Oil thus modified in its properties possesses a greater capillary constant or tension of surface, which differs the more from that of the unmodified oil the further the change has proceeded. The slightly modified oil (liquid 2) behaves towards oil which is either not at all modified or greatly so, as towards another liquid with another capillary constant.

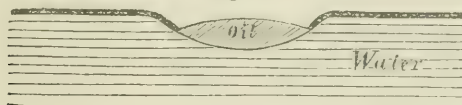
The lens-shaped oil-drop is formed of liquid 2, of oil not at all modified, or only slightly so (on account of the slow diffusion), which at the boundary between it and the air and the much modi-

* Franklin, *Phil. Trans.* 1774. p. 445. Mann, *Mém. d. Brux.* 1780, p. 255. Martin Wall, *Manch. Mem.* 1785, vol. ii. p. 419. Ben. Prévost, *Ann. de Chim.* (1) vol. xxi. p. 254; vol. xxiv. p. 31 (1797); *Gilb. Ann.* vol. xxiv. p. 158 (1806). Draparnaud, *Ann. de Chim.* (1) vol. xlvii. p. 304 (1803); *Gilb. Ann.* vol. xxiv. p. 143 (1806). Carradori, *Brugnatelli Giorn.* 1797; *Gilb. Ann.* vol. xxiv. p. 134 (1806). Link, *Gilb. Ann.* vol. xxiv. p. 121 (1806). Thomas Young, *Lect. II.* p. 659 (1807). Fusinieri, *Brugn. Giorn.* 1821; Frankenheim, *Cohäsionslehre*, p. 152. Weber, *Wellenlehre*, 1825, p. 78. Frankenheim, *Cohäsionslehre*, 1835, pp. 134-152. G. Hagen, *Abh. Berl. Akad.* 1845, p. 32.

fied layer of oil which covers the water or mercury has relatively a surface tension α_2 or α_{21} , both of which are less than the surface-tension α_1 of the free surface of the water or mercury covered with the much modified layer of oil. Liquid 2 will remain on the surface of liquid 1 as lens-shaped drops with various angles, or spread itself out, according to the magnitude of the constants α_1 , α_2 , and α_{21} , as experiment also shows.

Fig. 4 gives the cross-section of a drop of oil on a surface of

Fig. 4.



water containing oil. The form of the same changes with the quantity of oil already dissolved by the water and the nature of the film of oil which covers the water. The drop generally becomes flatter the longer it remains on the water.

By breathing on the oil-lenses swimming on water, there may be observed, when the surface is sufficiently mobile, a similar contraction of the oil-lenses and change of the angle to those I have described in § 27, when water lenses on mercury are breathed on. The phenomenon is accounted for here, as there, by substances which are contained in the form of vapour in the air expired and are condensed on the surface of the water.

As with water and mercury, so also with oil and water, thin layers of oil with circular holes occur whose angle is the same as that for a lens of oil on the same surface of water.

By means of the foregoing, the interesting phenomenon is explained which is observed when a drop of oil spreads itself out on a large surface of water, for instance, on a pond. A portion of the oil spreads itself out as a layer on the surface of the water to the extent of several square feet, and forms the most splendid interference-colours. The rest remains in the form of lens-shaped drops in the middle of the coloured oil-film. The latter gradually becomes thinner, draws itself together in a few places as irregular spots, and is gradually absorbed by the water. If the film of oil around the lens-shaped drop becomes infinitely narrow, then the latter suddenly spreads upon the free surface of the water; I might say the oil-lens explodes and forms a new film of oil of uniform colour or thickness, and from 2 to 3 inches in diameter. After this the process described above again repeats itself. This experiment sometimes succeeds with small drops of oil on a plate filled with pure water.

30. Besides those described in the foregoing paragraphs, a

great number of other experiments on outspreading have been made.

The appearance which the spreading usually presents is that particles of dust on the free surface of liquid 1 are repelled by it, or that liquid 2, spread out to a thin layer, shows Newton's coloured rings. By means of the foreign substances sticking to the particles of dust, however, the surface of the liquid is easily rendered impure; and optical methods are insufficient, as soon as the superposed layer of liquid becomes too thin, or the two liquids have nearly the same refractive indices.

As the substances brought into contact with one another, either through reciprocal solution or chemical influence, change their capillary constant, or even form entirely new chemical combinations, in which the atmospheric air and the moisture contained in it must be particularly considered, so in many cases a result can be observed to which the laws given in § 2 appear to be in contradiction. On a proper criticism of the methods used by different observers and the facts discovered by these methods, they will all be found in agreement with the theory.

A great portion of the experiments on spreading known to me can be estimated by the numerical data of Table X. § 10. The magnitude of the unknown capillary constants of the common surface of two liquids can in many cases be approximately estimated, if it be remembered that generally α_{12} is less the more liquid 1 is dissolved by liquid 2, or conversely.

For easy comparison of the theory with the results obtained by other observers, I give the capillary constant α at the mean temperature for a few liquids.

	α . milligrms.	Observer.
Ether	1·815	Wilhelmy*
Aqueous alcohol	2·589	Bède†
(spec. grav. 0·842)		
Soap-water ($\frac{1}{30}$ soap)	3·22	Plateau‡
Glycerine	5·143	Wilhelmy§

The series standing under the names of the respective observers are so arranged that the lower liquid always spreads out on the upper.

* Pogg. *Ann.* vol. cxxi. p. 52 (1864).

† *Mém. Cour. Sav. Étr. Brux.* vol. xxx. p. 160 (1860).

‡ *Recherches &c. 8^e sér. Mém. Acad. Brux.* vol. xxxvii. p. 91 (1868).

§ Pogg. *Ann.* vol. cxxii. p. 13 (1864).

<i>Draparnaud</i> *.	<i>Carradori</i> †.	<i>Link</i> ‡.
Water.	Water.	Water.
Alcohol.	Spirits of wine.	Fat oil.
Water.	Ether.	Petroleum.
Volatile oil.	Euphorbia juice.	Oil of turpentine.
Olive-oil.		Spirits of wine.
Spirits of wine.		
<i>P. du Bois-Reymond</i> §.		
Water.	Warm oil.	
Oil of turpentine.	Cold oil.	
Water.	Oil.	Glycerine.
Alcohol.	Alcohol.	Alcohol.
Water.	Oil.	Glycerine.
Ether.	Ether.	Ether.

31. The simplest case of the spreading of a liquid 2 on the free surface of a liquid 1 is when the two liquids are miscible in every proportion, and consequently in equation (1) α_{12} must be put = 0. Then the liquid 2 with smaller capillary constant spreads itself out on the liquid with the greater capillary constant; or, more accurately expressed, the greater tension of the surface of liquid 1 draws to itself, as it were, the particles of the surface of liquid 2 of lower tension of surface.

P. du Bois-Reymond || observed that a hot drop of a liquid, placed on the surface of the same liquid not warmed, likewise spread itself out there. As the capillary constant of a liquid diminishes with increase of temperature, $\alpha_1 > \alpha_2$, and the experiment can be predicted by the theory.

The same observer ¶ has called attention to the currents produced in the interior of liquid 1 by the spreading of liquid 2 when he placed a layer of oil upon a layer of water instead of upon a solid base. On the spreading of absolute alcohol on the surface of the layer of oil, vortical movements take place in the interior, and the water rises under the centre of the outspreading.

These secondary phenomena may be explained, as P. du Bois-Reymond ** has already observed, in that the alcohol carries away the layer of oil bordering on it, and appears every time more or less strongly impressed as soon as a liquid 3 is placed on the thin layer of liquid 2, which had spread itself out on the surface of a liquid 1. The liquid 3 spreads on liquid 2 as soon as

* *Ann. de Chim.* (1) vol. xlvii. p. 303 (1803). *Gilb. Ann.* vol. xxiv. p. 131 (1806).

† *Ann. de Chim.* (1) vol. li. p. 216 (1804). *Gilb. Ann.* vol. xxiv. p. 139 (1806).

‡ *Gilb. Ann.* vol. xxiv. p. 123 (1806).

§ *Pogg. Ann.* vol. civ. p. 196 (1858).

¶ *Ibid.* p. 197 (1859).

|| *Ibid.* p. 202 (1859).

** *Ibid.* p. 199 (1859).

$\alpha_{23} < \alpha_2 - \alpha_3$, and produces currents in the interior of liquid 2, and rolls off, as it were, the thin layer of liquid 2 from the surface of liquid 1. Hereby liquid 3 comes into contact with liquid 1; and it now depends on the magnitudes α_2 , α_3 , α_{12} , α_{23} , and α_{31} whether or not liquid 3 drives liquid 2 from the surface of liquid 1.

Moreover the more or less unknown friction of the particles of liquid against one another plays a part in this complicated process.

Hence it is entirely false to say that as soon as $\alpha_2 > \alpha_3$, liquid 3 drives away liquid 2. Thus, for example, olive-oil ($\alpha_3 = 3.76$ milligrms.) drives the alcohol ($\alpha_2 = 2.599$ milligrms.) from a surface of mercury upon which two drops of these liquids are placed together, although $\alpha_3 > \alpha_2$. The reason is, that for mercury and olive-oil $\alpha_{13} = 34.19$ milligrm., for mercury and alcohol $\alpha_{12} = 40.71$ milligrms., hence in the latter case is considerably greater. $\alpha_{23} = 0.226$ milligrm. for alcohol and olive-oil has, on account of its smallness, only a slight influence in this phenomenon.

For the case in which the capillary constant of the common surfaces can be neglected (that is, when the liquids brought together are miscible in every proportion, or the α with double index are small), the liquid with small capillary constant of the free surface will certainly drive away the liquid with greater capillary constant.

If liquids miscible in all proportions be so arranged that each one following spreads out on the free surface of the foregoing, then the same series will be obtained as if the liquids were arranged according to the magnitudes of their capillary constants.*

The foregoing consideration also holds good for the case in which a solid body 1 is substituted for liquid 1. Although the mobility of the particles of the liquid in contact with this solid body 1 is materially diminished, we must nevertheless, as I shall more closely argue presently, attribute a definite surface-tension to the liquid in contact with the surface of the solid body.

* Frankenheim (Theory of Cohesion, p. 142, 1835) has already remarked that the intensity with which a drop of liquid strives to spread itself out is greater in proportion as the specific cohesion of the liquid in question is less. Although, further on, he ascribes to the formation of vapour an influence on the spreading, still he appears to have had a correct, if not in many points a clear conception of this phenomenon.

More recently Lütge (Pogg. Ann. vol. cxxxvii. p. 377, subject 4 to 6) has enunciated the general law for any liquids, that the series of liquids arranged according to the magnitude of their capillary constants agrees with the order in which the liquids spread—and has at the same time stated “that the spreading takes place more distinctly the less the miscibility of two liquids and the greater the difference of their cohesions.” But as α_{12} appears to be less the more readily liquids 1 and 2 mix together, this statement is in contradiction to equation (4) § 25 of this memoir. The series only agree with one another in the case of the most perfect miscibility.

On these grounds I do not consider very trustworthy the method used by many observers, of placing thin layers of different liquids in contact with one another on glass, porcelain, or metal plates, and observing which liquid forces the other away. In many cases the tension of the *free* surface of the two liquids placed together so considerably preponderates, that liquid 2 spreads itself out on liquid 3 and forces liquid 3 away from the surface of the solid body 1, provided $\alpha_2 < \alpha_3$.

In this case, then, as the following summary shows, the order of the liquids arranged according to the magnitudes of their capillary constants, agrees with the order in which the lower liquid forces away a thin layer of the higher liquid from the surface of a glass plate. Whether there is a perfect agreement between the two series can only be judged from a simultaneous determination of the capillary constants of the liquids with which the experiments on spreading were made.

*Brugnatelli**

Olive oil.
Spirits of wine.
Oil of turpentine.
Ether.

Ben. Prevost†.

Sulphate of zinc. Common salt. Saltpetre. Glauber-salt. Blue vitriol. Sulphate of iron. Alum. Water. Fatty oils. Etherial oils. Alcohol. Ether.	} Aqueous solutions.
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Water.
Fatty oils.
Soapy water.

Carradori†.

Petroleum.
Oil of turpentine.
Ether.

Lüdtge§.

Chloride of iron.
Sal-ammoniac.
Water.
Blue vitriol.
Ammonia.
Muriatic acid.
Sulphuric acid.
Nitric acid.
Glycerine.
Potash.
Bisulphide of carbon.
Poppy-oil.
Acetic acid.
Plateau's soap-solution.
Oil of turpentine.
Benzine.
Alcohol.
Acetic ether.
Sulphuric ether.

* *Ann. de Chim.* vol. li. p. 216 (1804). *Gilb. Ann.* vol. xxiv. p. 136 (1806).

† *Ann. de Chim.* vol. li. p. 216 (1804). *Gilb. Ann.* vol. xxiv. p. 142 (1806).

‡ *Ann. de Chim.* vol. xl. pp. 1 to 32 (1801). *Gilb. Ann.* vol. xxiv. p. 180 (1846).

§ *Pogg. Ann.* vol. cxxvii. p. 369 (1869).

32. The beautiful experiment by Lüdgtge*, of making in Plateau's wire frame-work a film figure of water or oil (liquid 1) and causing it to be forced away by oil or soapy water (liquid 2), so that the films consist now of the latter liquid, may be explained by the secondary currents above-mentioned and connected with the spreading of liquid 2 in the interior of liquid 1.

Liquid 2 spreads upon one surface of liquid 1, provided $\alpha_{12} > \alpha_1 - \alpha_2$; it rolls off the other surface of liquid 1, which, as regards the nature of its surface, has been unaltered, breaks through this, and now forms a circular film, whose diameter gradually increases, as the diameter of the lens-shaped drop of water on mercury increased when oil was placed upon the boundary of mercury and water (§ 27). In this latter experiment also may be observed, under favourable circumstances, a perforation of the very flat lens-shaped drop of water or the formation of a circular film of oil in the interior of a film of water on the mercury.

Similar phenomena are seen on the surface of water when a drop of oil has been let fall upon it so as to be separated into many small drops in the water. One portion of the oil spreads itself out quickly, as was described above (§ 29), to a thin skin of modified oil with a greater capillary constant or tension of the surface than the original unchanged oil. If now the specifically lighter drops of still unmodified oil ascend in the specifically heavier water and break through the thin skin of oil, they form circular films on the surface of the water, whose diameters gradually increase until the influence of the water has also sufficiently modified the capillary constants of this oil. I should seek the cause of this phenomenon chiefly in the difference of the capillary constant of the free surface of the modified from that of the unmodified oil.

33. The decrease of the capillary constant or tension of surface when a liquid 2 spreads on the open surface of a liquid 1 can be proved by methods which differ from the above, and particularly from those given in the first four Sections of this memoir.

If water (liquid 1) be allowed to drop out of the end of a vertical sharply cut glass tube of r radius, then the weight of each drop will be $= \alpha \cdot 2r\pi$. By placing a small quantity of olive-oil or oil of turpentine (liquid 2) on the surface of the glass tube, which will then spread itself out on the surface of the falling drop, α will no longer remain $= \alpha_1$, but become $= \alpha_{12} + \alpha_2 < \alpha_1$, and the magnitude of the drop of water will considerably diminish. As the oil is soon carried off by the falling drops of water, the thickness of the coating of oil diminishes with the time, becomes $< 2l$ (than twice the radius of the sphere of action),

* Pogg. Ann. vol. cxxxvii. p. 465 (1869).

and the magnitude of the drops again increases. Owing to the difficulty of estimating the thickness of the coating of oil correctly, I have made no measurements with regard to it.

The longer the drops are in falling, the more time have they to condense substances on the surface which were contained in the atmosphere in the form of vapour, and just so much less must α or the capillary constant be found to be, as experiment also teaches. In the earlier* methods which I used with falling drops for determining the capillary constant of fused substances, this source of error had, on account of the high temperature of the fused substances, only a slight influence. The same holds good for measurements of flat drops of fused substances†.

If a horizontal capillary tube of radius r be fastened perpendicularly to the axis of an ordinary reflecting goniometer, and a small quantity of a liquid 1 (say, water) be placed in it, then the thread-like column of liquid between the two capillary menisci will be in equilibrium in all places of the cylindrical tube. If now a small quantity of a liquid 2 be placed upon one meniscus of the column of liquid 1, then by these means the capillary pressure $\frac{2\alpha_1}{r}$ on the top of the same will be diminished to $\frac{2(\alpha_{12} + \alpha_2)}{r}$, and the column of liquid pushed towards the other

end of the capillary tube. By a certain inclination v , measured on the goniometer, of the capillary tube, which depends on the length L and the specific weight σ_1 of the column of liquid 1, the column of liquid will again stand in equilibrium. Such a column of liquid with heterogeneous surfaces in a cylindrical glass tube behaves similarly to a column of liquid with homogeneous surfaces in a conical glass tube. The same difficulties as in the experiments described in Sections 3 and 4 stand in the way of obtaining a value for $\alpha_{12} + \alpha_2$ by measurements of the magnitudes v , L , and r , in that the changes of the angle cannot be estimated with sufficient facility. For lecture-room experiments, on the contrary, this method is well adapted to show the dependence of the capillary pressure or the tension of surface on the nature of the free surface of the liquid.

If a liquid 1 (say, water) be allowed to rise in a capillary tube in the usual manner, and then small quantities of a liquid 2 (say, olive oil or oil of turpentine) spread on the free plane surface of the liquid outside the capillary tube, then the elevation of the liquid in the capillary tube remains unchanged.

As the constant α_1 or H_1 (compare § 1) is very essentially modified by the introduction of this liquid 2, I should also have

* Pogg. Ann. vol. cxxxv. p. 642 (1868).

† Pogg. Ann. vol. cxxxviii. p. 150 (1869).

expected a change of the constant K_1 , and consequently a decrease of the elevation. As was pointed out at the commencement of this memoir, *i. e.*, no strict inference can be drawn regarding the magnitude of the perpendicular pressure K in the free plane surface of a liquid, as

$$K_1 - K_2 \text{ may be } = K_{12} = -K_{21}.$$

This relation, however, according to the numbers of Table X. § 10, which never give $\alpha_{12} = \alpha_1 - \alpha_2$, is very improbable; and on this account I am disposed to agree with the opinion of Thomas Young, according to which K_1 , the perpendicular pressure in a plane surface of a liquid, is to be put $= 0$.

Berlin, September 1869.

The foregoing memoir was already written, when, by the kindness of the author, I received the very interesting treatise of M. G. van der Mensbrugghe "On the Surface-tension of Liquids"*. It treats of motions on the free surface of liquids exhibited by other liquids, particles of camphor, or small solid bodies; and, from the same points of view as myself, he seeks the cause of these phenomena in the different magnitudes of the capillary tension of the surface of the liquid, which can be materially modified through the solution of small quantities of foreign substances. The greater or less tension of the free surface of the liquid is proved by a method discovered by the author, from the change in shape of the closed curves which are formed of thin threads swimming on the surface of the liquids. The treatise also contains a complete literature of the earlier works on the same subject.

As we proceeded in quite different ways independently of one another in the investigation of the same phenomena, I believed it was unnecessary to alter any thing in my written statement, so much the more so as, in my opinion, a complete understanding of the process of outspreading cannot be obtained without a knowledge of the magnitude of the capillary constant of the common surface of two liquids. Moreover all the experiments described by M. van der Mensbrugghe are in harmony with the foregoing theoretical considerations.

The principal results of the foregoing investigation may be recapitulated as follows:—

1. *At the common boundary of two liquids 1 and 2 a similar tension of surface takes place to that on the free surface of a liquid bounded by air.*

* G. van der Mensbrugghe "Sur la tension superficielle des liquides," *Mém. Cour. et d. Sav. étrang. d. Brux.* vol. xxxiv. pp. 1 to 67, 4to. See also p. 409 of *Phil. Mag.* for December, 1869.

2. The tension of the surface or capillary constant α_{12} of the common bounding surface of two liquids, multiplied by the sum of the inverse principal radii of curvature of a point of the bounding plane, gives the capillary pressure in the direction of the perpendiculars to the surface.

3. The magnitude of this capillary constant α_{12} does not admit of being directly determined from the capillary constants α_1 and α_2 of the free surface of the two liquids, but must be found by special experiments. It may have all possible values between 0 and $\alpha_1 - \alpha_2$.

4. If $\alpha_{12} = 0$, then the liquids 1 and 2 are miscible in all proportions, and no drop or bubble of one liquid is formed in the interior of the other. Moreover α_{12} appears smaller the more miscible the liquids are, and is always less than $\alpha_1 - \alpha_2$.

5. If three capillary surfaces meet together in a point, then the exterior angles of a triangle whose sides are proportional to the capillary constants of the three capillary surfaces are the marginal angles of the surfaces of the liquids.

6. A liquid 3 spreads out on the common bounding surface of two liquids 1 and 2 when $\alpha_{12} < \alpha_{31} - \alpha_{23}$.

7. A liquid 2 spreads out on the free surface of a liquid 1 when $\alpha_{12} < \alpha_1 - \alpha_2$.

8. If liquids which are miscible in all proportions, for which therefore $\alpha_{12} = 0$, be arranged so that each following one spreads out on the free surface of the foregoing, the same series will be obtained as if the liquids had been arranged according to the magnitude of the capillary constant of their free surface.

9. If a lens-shaped drop of a liquid 2 remains on the free surface of a liquid 1 without spreading itself out, then it is certain that in most, and probable that in all cases the free surface of liquid 1 is rendered impure by a thin layer of a foreign liquid 3. The effect of this foreign layer increases with its thickness up to a certain limit, which is equal to twice the radius of the sphere of action.

10. If a liquid 2 spreads out in a thin layer upon a flat drop of a liquid 1 in air or upon a flat air-bubble in the interior of the same liquid 1, then the vertical distance $K - k$ of the horizontal and vertical parts of the capillary surface diminishes. The new shape of the flat drops and bubbles may be calculated when α_{12} and α_2 are known.

11. If a liquid 3 spreads out on the surface of a flat drop of a liquid 2 in a liquid 1, it can only be said that the height $K - k$ of the drop of the liquid 2 diminishes. The change of form in this case cannot always be calculated beforehand.

12. The capillary constants of free liquid surfaces, determined on flat drops or bubbles, are greater than if calculated from the

elevations in newly drawn-out capillary tubes. The angle of liquids against pure glass surfaces is only in rare cases $=0$.

13. If two liquids are placed one above the other in a capillary tube, then the weight of liquid raised above the common level depends on the form of the free surface of the upper liquid o and the common surface of the upper and lower liquids o and u .

14. The weight of liquid raised above the common level is never determined by the lower liquid u alone, as Poisson says; in many cases, on the contrary, when the liquids o and u are miscible in all proportions, by means of the upper liquid alone.

15. The mean elevation of the liquids o and u admits of being estimated from the values α_o and α_{ou} measured on flat drops or bubbles when the angle made by the free and that made by the common surface of both liquids with the side of the tube are known. Only in a few cases is this angle 0° or 180° .

16. The observations of elevations in capillary tubes and on flat drops of a liquid or drops falling out of vertical tubes in air may easily give too small a value of the capillary constant, since foreign substances diffused through the atmosphere in the form of vapour are condensed on the curved capillary surface, and the thin layer of liquid arising therefrom diminishes the tension of the free surface. This source of error is greater at ordinary than at high temperatures, is more considerable in liquids with great capillary constants than in those with small, and accounts for the too small values of capillary constants found by earlier observers in some liquids, as mercury and water.

17. This condensation of moisture on the surface of liquids explains the different forms of lens-shaped drops of water on mercury and the so-called Moserian figures.

LX. On a Method of Fixing, Photographing, and exhibiting the Magnetic Spectra. By ALFRED M. MAYER, Ph.D.*

THE figures produced in iron-filings, when these are set in momentary vibration on a surface placed over a magnet, have received considerable attention from natural philosophers†. The geometrical discussion of these spectra made by Lambert, Roget, and others have developed their symmetrical properties, and thereby have evolved the law of that action which emanates from the magnet. De Haldat has used them as a means of exploring the distribution and intensity of the effect of juxtaposed magnets variously arranged. But above all have the researches of Faraday and W. Thomson on "the magnetic field," and on

* From the American Journal of Science and Arts, vol. i. April 1871.

† See a neat "Démonstration par le calcul des courbes magnétiques de la loi de l'inverse du carré de la distance," by M. Cellerier, published as a note on p. 592, vol. i. of De la Rive's *Traité d'Electricité*.

"the lines of magnetic force," given to these spectra, even when merely regarded as conventional symbols, an importance which has been fully shown (especially) by Faraday, who was guided by their consideration to some of his most important discoveries. They have thus risen to so high a theoretical importance that a method which will fix them without danger of distortion, photographically reproduce them and readily serve to exhibit them to the largest audiences, will, I imagine, be acceptable to both investigators and lecturers.

The only process of fixing these spectra known to me is that practiced by De Haldat and Faraday, which, however, is but an application to the magnetic spectra of the method previously invented by Savart for preserving the Chladni figures of vibrating plates. In this process the spectra, produced in the usual manner either on glass or cardboard, have pressed upon them a sheet of paper coated with mucilage, to which the filings adhere. In this operation of the transfer many particles are deranged from their positions, and the figures are yet more distorted by the shrinkage of the wet paper, and are therefore not fit to serve in measures of precision; while the impressions cannot be exhibited with much more facility than the originals.

My process is as follows:—A clean plate of thin glass is coated with a firm film of shellac, by flowing over it a solution of this substance in alcohol*, in the same manner as a photographic plate is coated with collodion. After the plate has remained a day or two in a dry atmosphere, it is placed over the magnet, or magnets, with its ends resting on slips of wood, so that the under surface of the plate just touches the magnet. Fine iron-filings, produced by "draw-filing" Norway iron which has been repeatedly annealed, are now sifted uniformly over the film of lac by means of a fine sieve. The spectrum is then produced on vibrating the plate by letting fall vertically upon it at different points a light piece of copper wire. The plate is now cautiously lifted vertically off the magnet and placed on the end of a cylinder of pasteboard, which serves as a support in bringing it quite close to the under surface of a cast-iron plate (1 foot diameter, $\frac{1}{2}$ inch thick) which has been heated over a large Bunsen-flame. Thus the shellac is uniformly heated, and the iron-filings, absorbing the radiation, sink into the softened film and are "fixed."

I generally allow the heat to act until the metallic lustre of the filings has disappeared by sinking into the shellac, and the film appears quite transparent. This degree of action is necessary when photographic prints are to be made from the plate; but when they are to be used as lantern-slides I do not carry the

* The shellac dissolved in strong alcohol is allowed to stand a week or more, and the clear supernatant solution is then decanted.

heating so far. After the plate has cooled, it is allowed to fall upon its ends on a table, so that any filings which have not adhered may be removed.

A short experience will give the proper strength of shellac solution to obtain a film so thick as *just to be sufficient to hold the filings*, and the requisite amount of heat to firmly cement them, without injuring the transparency of the film.

The plates can now serve (1) for the most accurate measures upon the magnetic field, (2) for a photographic positive, which in the printing-frame will produce the lines in white upon a dark ground, giving most beautiful and distinct impressions*; or (3) if it is required to exhibit these figures to an audience, the plates are provided with glass covers, kept from touching the spectra by intervening slips of cardboard, and there result "slides" in every way fit for giving a fine exhibition when the images are projected upon a screen. I have thus obtained images clear and sharp of over 12 feet diameter.

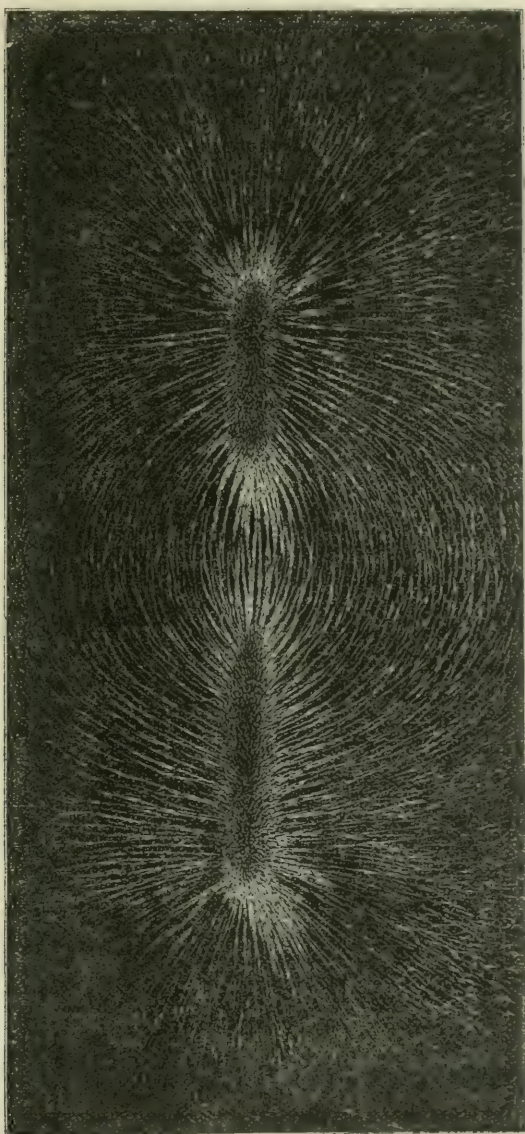
By this process many plates have been produced†, showing the action of single magnets of various forms, and of juxtaposed bars, as well as the effects of electric currents led by wires through holes drilled in the plates. Those exhibiting the inductive action of magnets on bars of soft iron and the interaction of magnets and electric currents are peculiarly interesting. An approximate representation of the resultant lines of the terrestrial magnetic action has been obtained by magnetizing *equally tempered* steel disks of from 2 to 3 inches, and even more, in diameter. The magnetic axis or axes of these disks are *predetermined* by making them the continuations of the axes of very powerful electro-magnets, terminated with cones of soft iron with slightly rounded apices. The arcs of the great circles including the terrestrial magnetic poles having been calculated, the axes of the electro-magnets are inclined to that angle, while the steel disk is held close to their poles. On passing the current the disk is magnetized, and we have an approximate representation of a section of the earth's magnetic effect. These results, when viewed as photographic prints or as exhibited by the lantern, are so beautiful and instructive as to appear to me to warrant this somewhat formal description of the process of their production.

December 1870.

* Photographic prints from a series of eight of these plates I have presented to:—Harvard College; American Academy of Sciences; Sheffield Scientific School; Columbia College; Stevens Institute of Technology, Hoboken; Lehigh University, Pa.; American Philosophical Society; Franklin Institute; Peabody Institute, Balt.; Smithsonian Institution; Chicago Academy of Sciences; and to the University of Virginia,—where they can be examined by the readers of this paper.

† Several of these are 16 inches long by 10 wide.

Magnetic Lines of Force (from a photograph by the Author)*.



[* For the loan of this woodcut we are indebted to Messrs. Longmans, the publishers of Professor Tyndall's recent volume, 'Fragments of Science,' of which it forms one of the illustrations.—W. F.]

Since sending the above to press, I have subjected plates coated with "sensitized" collodion to the action of the magnetic field. I had hopes of thus obtaining a *physical* impress on the plate which would appear on flowing the "developer." Sensitized films on glass and on iron plates were placed over and between the poles of an electromagnet with cores 1·7 inch in diameter. Some plates were developed after removal from the magnet, others while under the magnetic action (with and without the light having acted upon them); but no trace of effect has been detected.

I had also imagined that the magnet's action should have placed the affinities in a more unstable condition, so that the film would rise in sensitiveness after exposure in the magnetic field; but this also I could not detect; nevertheless I have not given up the supposition that some action will be evolved when more appropriate films, far higher magnetic action, and more delicate measures of actinic effect are used.

February 13, 1871.

LXI. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from p. 321.]

December 7, 1870.—Joseph Prestwich, Esq., F.R.S., President, in the Chair.

THE following communications were read:—

1. "On Fossils from Cradock and elsewhere in South Africa." By Dr. George Grey.

From the Karoo-beds, Dicynodont fossils and the jaw of a Reptile, *Estheria*, and some coal and coal-plants (*Lepidodendron*, *Sigillaria* &c.) were the chief specimens noticed by the author. Some *Stigmara* from the Old Coal of Lower Albany, and gravel and miscellaneous minerals from the Diamond Fields, formed part of the collection.

2. "On some points in South-African Geology."—Part II. By G. W. Stow, Esq.

This paper commenced with a detailed account of the Forest zones, coal, and other strata of the Karoo formation, as seen in sections in the Winterberg and Stormberg. The author particularly pointed out the position of the Fern-beds at Dordrecht, of the Reptilian remains found on the Upper Zwartkei, and of the Coal on the Klaas Smits River. He next referred to the climatal changes of South Africa, as indicated by its geology and fossils, particularly the Karoo-beds, the *Enon* conglomerate, the *Trigonia*-beds, the several Posttertiary shell-beds, and especially the present surface conditions, which he regarded as due to ice-action, as evidence of which he

adduces *roches moutonnées*, moraines, basins, and striae, both north and south of the Stormberg, in British Kaffraria, and even in Lower Albany. He concluded with remarks on the probable succession of periods, and on the former existence of a great southern continent.

3. "On the Geology of Natal, in South Africa." By C. L. Griesbach, Esq., Corr. Memb. of the K. K. geologischen Reichsanstalt, and of the K. K. geographischen Gesellschaft, Vienna.

The author commenced by describing the physical geography of Natal, and then indicated the characters and distribution of the rocks which occur in that country. He stated that the granitic and gneissic rocks do not form the most prominent elevations, but they appear chiefly in the lower parts of river-valleys, and sometimes in small hills. Mica-schists and slates are found associated with the granites. The great plateaux consist of an undisturbed sandstone, which the author identifies with the Table-mountain Sandstone, and which lies horizontally upon the granites and old slates. The tops of many of the table-mountains in Natal are crowned by beds of dark basaltic greenstone. The Karoo formation, which lies in part upon the Table-mountain Sandstone, consists of a vast series of sandstones and shales, some of the latter containing beds of coal. The author agreed with Mr. Tate in regarding these beds as of Triassic age. At the base of the Karoo formation the author described a boulder-bed, which he was inclined to identify with the rock described by Mr. Bain as "Claystone porphyry;" and through this greenstone has forced its way. On and near the coast of the southern part of Natal, some sandy marls and sandstones belonging to the Cretaceous series were said to occur; the author gave lists of fossils obtained from these deposits, which he identified with the Trichinopoly series of India. Several of the fossils were described as new species. The author considered that the evidence adduced indicated that, after the development of the Table-mountain Sandstone, Africa and India formed parts of one continuous continent, afterwards covered by the Cretaceous sea. The area now covered by the Indian Ocean was the basin of a large series of lakes; and this condition persisted through a long period of tranquillity, lasting through the Triassic to the Upper Jurassic age. The greater part of this continent was then depressed and covered by the shallow Cretaceous sea. The economic mineral products of Natal were mentioned by the author, who referred to the occurrence of graphite, coal, gold, and copper.

4. "On the Diamond-districts of the Cape of Good Hope." By G. Gilfillan, Esq.

Mr. Gilfillan described his going through Colesberg to Hopetown, and thence across the Orange River to Backhouse; and then, after crossing the Vaal, up its right bank as far as Lekatlong. He noticed such diamonds as he saw or heard of, and described the locality as being thickly coated with sand, diamond-bearing gravel, and tufa, hard blue shales occurring here and there in protruding hills.

LXII. *Intelligence and Miscellaneous Articles.*

ON BAROMETRIC COMPENSATION OF THE PENDULUM.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

REFERRING to Professor Heller's article "On a Barometer without Mercury" in the last Number of the *Philosophical Magazine*, page 401 &c., I may mention (as another possible adaptation of the same fundamental principle to a different purpose and in a different form) a plan which occurred to me some time since for correcting the barometric inequality of the ordinary clock-pendulum, by affixing to the pendulum-rod, produced upwards, a body of the same volume as the pendulum-bob, but of very small weight. This implies that the upper part of the pendulum-rod has the same length as the lower part; if it be shorter, the volume of the light body must be larger.

It appeared to me, however, probable that, in practical use, inconvenience might be produced by this construction, and I never actually made a trial of it.

Royal Observatory, Greenwich,
May 3, 1871.

G. B. AIRY.

ON THE USE OF THE SPECTRAL APPARATUS IN THE QUANTITATIVE DETERMINATION OF COLOURING-MATTERS. BY K. VIERORDT.

In the spectral apparatus hitherto in use the slit is bounded by two plates, a fixed and a moveable one. For the present purpose the moveable plate is divided into two parts, an upper and a lower one. Each of these plates is provided with a fine micrometer-screw, by means of the milled head of which the breadth of the slit may be accurately determined. If both the halves, the upper and the lower, are of exactly the same width, the upper and the lower half of the spectral field of view is of exactly the same intensity in every part. But if a transparent coloured medium is brought in front of the upper slit (a coloured glass for instance, a thin lamina of a coloured body, a coloured organic structure, or, finally, in a small glass trough with parallel sides a solution of any coloured substance or the colourless solution of a strongly fluorescent body), the spectrum is divided into two halves, one above the other, of different luminous intensities—into the pure spectrum of the source of light produced by the part which remains free, and into the spectrum modified by the interposed coloured body.

I may mention another accessory arrangement which I have introduced into the spectrum-apparatus, which I can also recommend for other spectrum-analysis purposes than the present, namely an arrangement in the eyepiece of the observing-telescope which renders it possible to stop all parts of the spectrum excepting that investigated. By this arrangement very feeble coloured lines of the spectrum of chemical elements may be observed with great accuracy. For details I must refer to my work, which has just been published

(Laupp, Tübingen), 'On the Application of Spectrum Analysis to the Measurement and Comparison of the Intensity of Coloured Light,' in which the spectra of solar light and of various terrestrial sources of light are photometrically examined, and moreover measurements of the luminous intensity of the bright lines of the spectra of some chemical elements are communicated.

The first problem in the quantitative determination of coloured bodies by spectrum-analysis (a problem which has not hitherto been put in chemical laboratories) consists in measuring the *absorption of light in any given* region of the spectrum by a transparent body brought before one half of the slit, the rest of the spectrum being stopped off in the manner described. This is very simply effected by narrowing the fore part of the slit by means of the corresponding micrometer-screw until the luminous intensity is the same in the upper as in the lower half of the spectrum investigated. If the luminous intensity in the part investigated is very great, the free half of the slit is first covered with a smoked glass of known darkening-power, and then perfect equality of luminous intensity is produced by altering the width of the slit.

Equality in luminous intensity in the two halves of the spectrum is very quickly produced; and thereupon the intensity of the light which remains after traversing the coloured body brought before the slit is directly found in a percentage. In some investigations the measurement of the photometric values of the region investigated is also desirable; this is effected by the method described in the above-mentioned paper.

As the eye can discriminate very small differences in the intensity of monochromatic light, the method possesses all the guarantees of an accurate objective measurement. It thus furnishes a simple means of measuring the absorption of light in all regions of the spectrum; it is also suited to determine the contents of any given solution of a colouring-matter.

To the definition of the coefficient of the absorption of light, as well as of the coefficient of extinction (E) of a solution, moreover of the coefficient of absorption (A) of the dissolved active absorbing body, the following is to be added: the first and third of these coefficients correspond to the definitions which have long been used, while the coefficient of extinction is taken in the well-known sense introduced by Bunsen (in his photochemical investigations).

A definite relation between the coefficient of extinction E and the concentrations of variously concentrated solutions of one and the same coloured substance may be anticipated; $\frac{C}{E}$ is, in fact, nothing more than the coefficient of absorption A of the dissolved substance, corresponding to the older definitions.

If, then, for any given single place of the spectrum the coefficient of absorption of the coloured substance (that is, the value $\frac{C}{E}$) and hence the coefficient of extinction of a single solution of previously known concentration has been determined, any unknown concentra-

tion C of the same solution may be found by the formula $C=AE$,—that is, by simply multiplying the coefficient of absorption of the substance (which has once for all been determined) by the coefficient of extinction of the solution (to be measured on the spectrum-apparatus). The coefficient of extinction is the negative logarithm of the luminous intensity which remains after traversing a layer of the absorbing medium a centimetre in thickness.—*Berichte der Deutschen Chemischen Gesellschaft*, No. 6, 1871.

ON THE ZODIACAL LIGHT. BY A. C. RAYNARD, ESQ.

The conviction that the accumulation and comparison of evidence on a subject so interesting and yet so little understood as the zodiacal light cannot but be useful, induces me to submit the following observation to the notice of the Astronomical Society:—

On the evening of the 19th of December last the detachment of the Eclipse Expedition encamped at Agosta had an opportunity of examining a particularly brilliant display of the zodiacal light, stretching to some 80° from the sun's place: its contour was of a somewhat conical form, blunted at the apex, the semi-vertical angle of the cone being about 12° . Its light was apparently white, and was undistinguishable in point of colour from the light of the Milky Way, which also shone out with considerable distinctness upon the night in question. I was anxious to see if any polarization could be detected, and for this purpose made use of a Savart, and at first thought that faint lines were visible, indicating polarization in a plane through the sun; but not being at all sure of my observation, I asked Mr. Burton, who was formerly assistant to Lord Rosse, and has very keen eyesight, to look through the instrument: he at once said that he distinctly saw bands brightest when the Savart was turned so that the direction of the bands passed through the sun, and that the centre band was black; he also saw the bands perpendicular to the sun's direction, but could not determine the nature of the centre one. I should mention that the Savart was so set as to give a black centre when the bands were parallel to the plane of polarization.

I then asked Mr. Burton to see if he could detect any bands upon the sky away from the zodiacal light, but he was unable to do so even at 90° away from the sun's position.

I again took the instrument, but was unable to see any bands myself. On the next night the zodiacal light was again visible, but with considerably less brilliance than before. Father Secchi observed it with a Savart, and thought that he detected faint bands, but said that he could not be sure of his observation.

If other observations should confirm those of Mr. Burton, we shall be in possession of proof, not only that the zodiacal light consists of matter which reflects the sun's light, but that

(1) That matter exists in particles so small that their diameters are comparable with the wave-lengths of light; or

(2) It consists of matter capable of giving specular reflection.—*Monthly Notices of the Royal Astronomical Society*, March 10, 1871.

THE
LONDON, EDINBURGH, AND DUBLIN
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

SUPPLEMENT TO VOL. XLI. FOURTH SERIES.

LXIII. *On Glaciers.* By ALBERT HEIM, of Zurich*.

[With a Plate.]

THE glacier-clad mountains of my native country, especially when glistening in the rays of evening, exerted on me, from my childhood, a powerful attraction; and this was afterwards enhanced by their scientific interest. The present memoir contains individual observations and reflections upon certain phenomena of glaciers—the result of the magnificent days I have spent in the glacier-region under all conditions of the atmosphere, of the study of the literature of the subject, and of some experimental researches.

1. *The Glacier-Grain.*

The observations of the capillary fissures which run through the ice of glaciers, and form the boundaries of the glacier-grain, and the reflections suggested by them, especially as regards their origin, occasioned a strong controversy among those who 30 or 40 years ago devoted themselves to glacier-investigation; and no one conquered. In the more recent works the question is mostly passed over. Even Professor Tyndall ('Glaciers of the Alps,' pp. 338, 339) devotes only a few lines to the capillary fissures, and does not mention the glacier-grain. So much the more were we surprised at a memoir by M. Grad, which appeared in 1867, in the *Comptes Rendus*, in which he advances precisely the view which Hugi forty years previously had so energetically maintained. It is not clear from the memoir whether the author was acquainted with Hugi's writings, or whether by his own observations he has independently arrived at the same result. This view may be briefly expressed thus:—Direct observation teaches that the grains (or crystals) of the glacier are the further

* Translated from a separate copy, communicated by the Author, from Poggendorff's *Annalen, Ergänzungsband* v. pp. 30–63 (1870).

developed granules of the *névé*. This development, consisting in enlargement and (according to M. Grad) superadded "crystalline orientation" of the granules of the *névé*, it is to which all the phenomena of the movement of the glacier are to be referred. Thus Hugi and M. Grad. The latter remarks, "Professor Tyndall derives all the phenomena of the motion from pressure; but this is erroneous;" of this erroneousness, however, he gives no proof, and as little on behalf of his own view. Resting on the observation that the glacier-grain is larger towards the lower part of the glacier than further up, and that in the direction of the lower part of the glacier the structure of its ice more and more approaches the crystalline condition of water-ice, while the axes of the crystalline grains arrange themselves all parallel and perpendicular, he simply says it is so.

Still more diverse are the results of the researches intended to answer the question whether the network of capillary fissures runs through the whole body of the glacier. Agassiz found the capillary fissures even in the most compact glacier-ice. Schlagintweit infiltrated them with solutions of chromate of potassium to the depth of 60 and 80 metres beneath the surface of the glacier. MM. Bertin, Grad, and Dupré obtained results agreeing with the above. On the contrary, Hugi absolutely denies the penetrability of the inner ice of the glacier; and according to Mr. Huxley's experiments, in sound unweathered glacier-ice no capillary fissures exist. All have experimented very conscientiously; but the results are diametrically opposite. Whence comes this?

If the capillary fissures exist throughout the glacier-ice, but, as must be the case in "sound blue" ice, are filled with water, infiltration experiments cannot lead to any result; for there is nothing to cause the water to give place to the coloured liquid, unless we have to do with a layer of ice beneath which the liquid can freely run away, and on which there is the pressure from above of a considerable column of the coloured liquid. It is still more difficult to conceive that the infiltration through capillary fissures, which the other observers assert that they have seen, was a mere delusion; for it is agreed that they also operated on "sound" glacier-ice. In the parts of the glacier where great pressure prevails, and so everywhere in the "*structure-mill*" of Professor Tyndall, the water in the capillary fissures, supposing these to go through the whole glacier, is partly pressed out. In these parts it will be impossible for the infiltration-liquids to penetrate; this will more easily be possible where the ice is subjected to less pressure—for example, above declivities where transverse fissures make their appearance, and in bends of the valley, on the convex side of the body of the glacier. The dif-

Fig 1



Fig 2



Fig 3



Fig 4



Fig 5



Fig 6

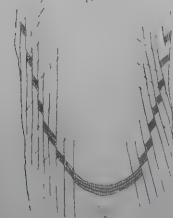
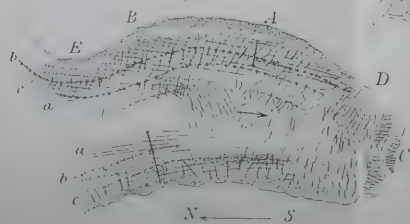


Fig 7



N S

Fig 8

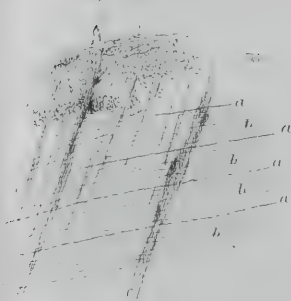


Fig 9



Fig 10



Fig 11

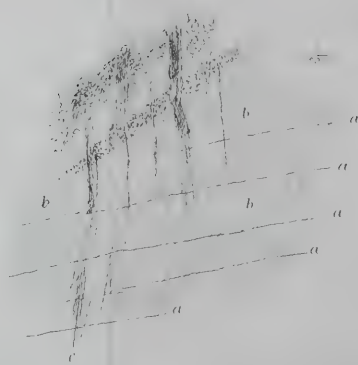


Fig 12

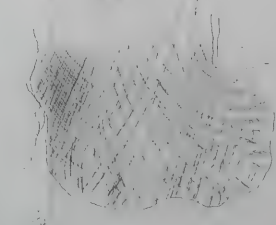


Fig 13



Fig 14

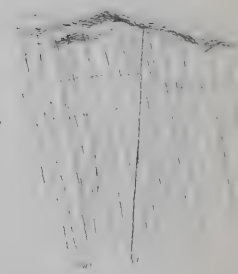


Fig 15

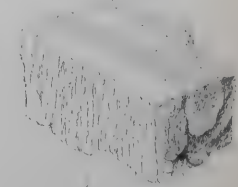


Fig 16



East

ferent results of infiltration experiments probably stand in direct relation to the parts of the glacier where they were carried out.

That even the most solid, transparent, blue ice from the interior of the glacier, in the warmer air, and especially through the operation of the solar rays, becomes white through a network of fine fissures becoming visible, which everywhere divide the ice into irregularly formed grains of a certain constant magnitude in any one locality, is an observation which no one has yet disputed and which any one can repeat. But why does not this happen to common ice? How could heat call forth such a granular structure, if it were not already sketched out, even in the "sound," compact ice? How it is present in the "sound" ice—whether in the form of actual capillary fissurelets filled with water, or whether, from some unknown cause, the ice more readily liquefies on surfaces irregularly permeating the mass, or how otherwise it may be imagined—whether capable of infiltration or not is to us indifferent,—it is sufficient that a similar granular structure to that which we observe on the surface goes through the whole mass, or at least is traced out therein.

Attempts have been made to explain how this granular structure arises. The unproved view of MM. Hugi and Grad, above quoted, that the glacier-grains are the further developed granules of the *névé*, is not what they think, a something taught by direct observation, but an hypothesis. (I shall return to this question.) We will for a few moments adopt it, and found upon it a little calculation, which will the soonest put it in the right light.

In glaciers of about 8000 metres length (for example, the Hüfi Glacier, Rhone Glacier, &c.) the glacier-grain grows from the size of a wheat-grain to at least the size of a walnut (in larger glaciers, according to Hugi, it may become "even 5 or 6 cubic metres in volume"), thus from about 3 millims. in diameter to 30; the corresponding volumes are from 3^3 to 30^3 , or (which gives the same proportion) from 1 to 1000. And this enlargement of form takes place throughout the mass of the glacier, and not merely in the layers nearest to the surface. In the time necessary for a point at the upper end of the glacier to arrive at the lower, the glacier, but for the superficial melting away (*ablation*), would become 1000 times its original volume; the ablation, then, must not only, in just this time, carry away in water the mass of ice present, but also compensate for the thousandfold increase. Although we shall apply our calculation to the Aletsch Glacier, the length of which amounts to more than twice 8000 metres, we will yet, in favour of the hypothesis, content ourselves with a simple increase of volume to 1000 times instead of 2000. The numbers we shall use will be only approximate, always rounded off in favour of the hypothesis.

The mean annual rate of motion of the Aletsch Glacier is about 40 metres. Measured from the foot of the Dreieckhorn and Faulberg (where the glacier, properly speaking, begins) onwards, it is 17000 metres long. We will now, as regards alteration in volume, consider the glacier below the transverse section situated at the above-designated place, until it is all melted, and thus our transverse section, slowly travelling with the ice, has arrived at the lower end. For this about $\frac{17000}{40} = 425$

years are necessary. The volume of our glacier-tongue may be estimated as about 6150 million cubic metres; of course the number is not certain to 100 millions more or less. This mass of ice, together with its increase through the growth of its granules, actually turns to water in 425 years by ablation. The melting of the glacier beneath through the warmth of the ground is doubtful—at any rate, vanishingly small. The vertical sinking of the surface through melting away amounts to an average of about 3 metres yearly: it was measured by the constantly increasing protrusion of stakes which had been driven into the surface, or by the relative elevation of parts artificially sheltered from ablation. The surface of ablation in the first of the 425 years, measured on the Federal atlas, is 25,000,000 square metres. In the second, the transverse section forming the upper boundary of our glacier-mass has moved 40 metres down the valley; here the glacier is 1600 metres wide; thus the surface of ablation is, for the second year, $40 \times 1600 = 64000$ square metres less than that of the first. Just so in the following years it becomes continually less the further our transverse section travels. At the end of the 425 years it reaches 0. The surface on which, during the 425 years, the vertical ablation of about 3 metres yearly has operated, and effectually melted the mass of the glacier, is the sum of all these 425 unequal surfaces belonging to the individual years. If the surface of the glacier were exactly rectangular, as broad below as above, this sum would be 425 times half the surface of the first year, consequently

$$= 425 \times 25000000 \times \frac{1}{2} = 5312500000$$

square metres. Were it exactly a triangle (of which the base, the greatest breadth, = 1800 metres, and the height the length = 17000), the sum in question would be

$$= \frac{1800 \times 17000}{2 \times 425^2} (1^2 + 2^2 + 3^2 + \dots + 425^2)$$

or = 2175000 square metres. In reality it lies between; we will take the mean—namely, 3740 million square metres. To mark on the map the situation of the upper limit for each year,

to measure the surface below this, and to sum up the 425 surfaces would be a needless labour, because all our other numbers are not correspondingly accurate.

We have now the equation:—Surface multiplied by ablation equal to original volume times x . If the hypothesis of Hugi and Grad is correct, we must obtain for x above 1000; if no real increase of mass takes place in the ice, a value near 1. Inserting our values,

$$3740000000 \times 3 = 6150000000 \times x, \therefore x = 1.8.$$

That is, in order to compensate for the thousandfold increase of volume which would ensue from the growth of the granules, the ablation would have to be nearly 1000 times as great as it is; otherwise, instead of the volume of the glacier having quite vanished at the end of the 425 years, it would be nearly 1000 times as great as at the beginning.

From this calculation it appears to me certain that, if it were true that the glacier-grains are the developed *névé*-granules—if it were true that the large glacier-grain of the lower regions originates from the smaller granule of the upper through growth produced by the freezing upon it of infiltrated water, then the glacier would become larger, it would never come to an end, and at the present time the whole earth must be lying under a deep covering of ice.

In the above we have rounded off all the numbers in favour of the hypothesis of Hugi and Grad, and hence have found for x a greater value than 1; but we could take them still more favourably for that theory, and yet the result would be strikingly adverse to the hypothesis.

But the old dilatation theory must accept an increase of mass through the water freezing in the capillary fissures. M. Grad insists upon the dilatation theory, without adducing a single experiment to confute the numerous objections which, years before, were advanced against it. The measurements made by himself and M. Dupré in 1869, on the Aletsch Glacier, contain evidence against the dilatation theory; for he there finds that the lower part of the glacier moves considerably more slowly than the upper. As Forbes acknowledged, according to the dilatation theory, that should be exactly reversed, since we have to regard the upper end of the glacier as a fixed point of support for the glacier-tongue, and the dilatation must sum up to a continually greater motion for a point the further it is removed from the beginning of the glacier.

Some other facts can be adduced which are opposed to the derivation of the grain of the glacier from that of the *névé*. The distention occasioned in the mass by the growth of the granules

could not but show itself in the form of the glacier; but only a shrinking through melting is to be recognized therein, never a swelling.

We very often find isolated air-bubbles enclosed in the large, splendidly transparent glacier-grains, and sometimes, indeed, quite in their centres, sometimes excentric. In the fine *névé* the air is found only *between* the granules; and if these were gradually to grow larger by water freezing on them, the air would remain between the grains. I cannot conceive how it could be enclosed in them. On the contrary, in thawing snow it is immediately evident that it becomes large-grained through single minute crystals, afterwards single granules, uniting to form one, and consequently that a larger grain of the *névé* is not a small one increased in size, but corresponds to several small ones.

That the grain of the upper part of the glacier is smaller than that of the lower it is easy to see; but, often as I have sought for it, I have never been able to trace the transition from the *névé*-grain to the glacier-grain. Where the *névé* ends and the clear ice, though rich in bubbles, appears at the surface, this shows no distinct granular structure; the glacier-grain begins to make its appearance (at first rather small) further down the valley. Also the increase of size of the grains as the end is approached seems to me to take place not at all so regularly as has been asserted. For a long time I thought my own observation was at fault, until the above calculation reassured me.

How could the fine divisions between the individual *névé*-granules be preserved for two or even three or more centuries, often under strong pressure, without being destroyed by regelation?—while by this process the mightiest crevasses may disappear and scarcely leave a trace, and two pieces of ice, gently pressed together for a few days, so regelate that the piece, on being fractured, will not break again at the same place.

The network of fissures, or the structure of the glacier-ice which in warm air gives rise to that network, is necessarily always passing away and almost simultaneously being formed afresh; for, as is proved by our calculation, the glacier-grains (bounded by the capillary fissures) of the higher and lower parts of the glacier do not correspond to each other.

It was already conjectured by Forbes that these fissures might be divisions produced, or at least preserved, by pressure and motion.

When we take a piece of ice from the surface of the glacier and divide it into its grains, we are struck by the fact that they do not all separate with equal facility. While the fissures circumscribing the grains are partially visible through the air which

has penetrated them, and appear silvery white, and along these the grains can be easily separated (primary capillary fissures), irregular surfaces of division transverse to the grains come into view on the application of greater force. The latter are surfaces of division already traced in the substance, not new surfaces of fracture; they often help us to divide a grain into several pieces; by way of contrast to the above-mentioned we will name them secondary capillary fissures. It is natural to suppose that these secondary fissures are old primary ones not yet quite regelated, and that in these two forms we have surfaces of division of the glacier-grains in different states of development. The primary capillary fissures are mostly smooth, curved or plane; the secondary show for the most part (perhaps always) fine, acute elevations which accurately fit into depressions in the adjacent faces, often all elongated in one direction; and then the faces exhibit parallel wrinkles.

Between the explanation of glacier-motion by pressure, as given by Tyndall, and the continual renewal of the network of fissures there is, it seems to me, a deep and intimate connexion. Professor Tyndall says, in relation to his experiments, "The ice, in changing its form from that of one mould to that of another, was in every instance broken and crushed by the pressure;" and then, with respect to the glacier, continues, "but suppose that, instead of three moulds, three thousand had been used,—or, better still, suppose the curvature of a single mould to change by extremely slow degrees—the ice would then so gradually change its form that no rude rupture would be apparent. Practically the ice would behave as a plastic substance."

I believe that just what is here called "rude rupture" takes place also in the glacier, and occasions the breaking up of the ice into grains. Glacier-ice is so brittle that I cannot imagine an alteration of its form without a continual shattering into fragments (without "rude rupture"), even if the form of the mould into which it is pressed be ever so slowly altered. In the glacier also the ice mass is every moment broken into fragments (the glacier-grains), and these are again cemented by regelation; both processes proceed simultaneously and uninterruptedly throughout the mass of the glacier—and, indeed, the more actively the quicker the motion, and the more irregular the form of the valley, to which the glacier must bend—in short, the greater the relative motion of neighbouring portions of the ice.

We cannot expect to be able to find, in support of this view, forms of conchoidal fracture in the grain of the glacier. The concentric streaks which characterize that are only grouped around certain pronounced points of action of the force; while the pressure in the glacier takes place almost uniformly through-

out the mass : we have here an infinite number of mutually equivalent points of action for the force.

If the above explanation of the origin of glacier-ice is correct, it must be possible to produce also in compact water-ice, artificially, a breaking up into fragments analogous to the glacier-grains ; and in this I have actually succeeded. Had Professor Tyndall, in his experiments on the moulding of ice, not put in operation so strong a pressure that the ice must have instantaneously burst into fine powder, had he not suddenly compelled it to so great a change of form, and had he experimented on larger masses, a granular structure would have been visible in the moulded ice. His glacier-grains were as fine as powder, and hence escaped observation.

In a block of wood a tray-shaped depression was cut about 1 foot broad, and 6 inches long ; the radius of its curvature was 2 feet 6 inches ; and a piece of wood was cut so as to fit into the cavity. Between these two parts of the mould a flat plate of river-ice, beautifully clear, about 2 inches thick, was placed, exactly as long and broad as the cavity ; and the whole was firmly enclosed with boards to prevent the escape of the ice. A stroke with a weight-stone of half a hundredweight, brought down upon it, was sufficient to crush the flat plate of ice into the shape of the cavity.

A main imperfection in this experiment is that here a nearly equal pressure does not, as in the glacier, operate simultaneously on every part of the mass to be moulded, but the pressure acts on different parts of the plate of ice in succession, not simultaneously, although within the brief time of the stroke ; consequently the effect cannot be equal throughout the entire mass as in the glacier. The experiment would be improved if the shape of the cavity could be slowly altered—perhaps by the plate of ice, squeezed between two flexible steel plates instead of in a wooden mould, being slowly curved. On account of the imperfection indicated, the fragments exhibit conchoidal fracture (concentric streaks).

Between the two parts of the mould, and bent to its concavity, there now lay a mass of ice divided by cracks into grains. These varied in volume from not quite a cubic centimetre to about six times as much ; and between them there lay frequently fine splinters of ice. In one case the ice mass thus moulded, lying in the hollow, was kept cold, and from time to time sprinkled with water ; in a second case it was placed where in the daytime the sun produced a slight melting, but during the night the temperature sunk considerably below 0°C . ; and in the third, it was placed, under a continuous pressure of half a hundredweight, for two days in a room, the temperature of which, except during

the night, remained between 0° and -2° , and only rose a little above 0° in the afternoon. In the first two cases the circumstances were similar to those at the surface of a glacier; in the third, more like those which operate on the internal ice of it.

In each case I obtained a mass of ice apparently pretty compact; only a part of the fissures were still visible, though certainly not distinct. I could perceive no difference between its appearance and that of the surface-ice of a glacier in the early morning, before the ablation commences—at least, as far as I still had the latter in my memory. On applying a little force, the whole could be severed by the hand into its grains. Placed in the warm room, or in the midday sun, the network of fissures became somewhat more distinctly visible, and the grains could now be more easily separated. Their surfaces had no longer the form of conchoidal fracture, but were rough with irregular elevations and depressions, exactly as in the secondary capillary fissures, except that the unevennesses were finer and more numerous.

In these experiments, immediately after the breaking, conchoidal surfaces only were seen; after regelation and partial soaking had operated, only capillary fissures of the second order. Because all these processes take place simultaneously in the glacier, therefore we find also their effects together in the glacier-grain.

From the observations adduced it follows that the secondary fissures originate from the primary by a secondary process, which can be no other than regelation and soaking, because no other intervened. Under the given circumstances, these processes produce a wrinkling of the surface in the originally conchoidal (in the glacier, flat) capillary fissures. Some experiments for the purpose of deciding whether it was more the one process or the other could be brought to no further conclusion.

On the banks of the Havel and of the Müggelsee, near Berlin, I found in the thawing crust of ice a division into irregular vertical prisms (often trilateral). The ice had been formed during a fall of snow. The prisms were partly separated by interspaces, and partly adherent; but the surfaces of nearly all of them were finely wrinkled, reminding one of the secondary capillary fissures. From the infiltrated water produced by the melting, furrowed surfaces also occurred, which must not be confounded with the plaited ones.

The tray-shaped granular ice plate obtained in the experiments could, within certain limits, be bent exactly as is the case with the granular plates which, on hot summer days, can often easily be separated from the borders of the crevasses and parallel to them (reminding one of itacolumite). When thus carefully

bent, the same noise, proceeding from the sliding on each other of the grains, was audible as in plates of real glacier-ice. When forcibly bent more than the movement of the grains permitted, fracture gave origin to new primary fissures. It is, in all probability, the reciprocal friction of the grains, some of which are strongly pressed against each other, together with the "rude rupture" (or the origination of new fissures), that produces the noise often perceived (and first noticed by Agassiz) in glaciers when otherwise deep stillness reigns—for example, in the night or early morning, ere yet the brooks from the melted ice have begun to purl and babble.

The aspect of the granular ice obtained from the experiments is, at the first glance, exactly that of genuine glacier-ice, the sole difference being that there the size of the grain is more regular than could be obtained in our little experiment.

Could we suddenly take away the property of regelation from the glacier-ice, the glacier would behave in its movements like sand, earth, or a heap of gravel*.

When, in the above experiments, a weaker pressure (stroke) was applied, the plate of ice separated into not very numerous fragments or grains. The stronger the pressure, the more accurately must the ice conform to the cavity, and the smaller the grains. Each new fissure begins and proceeds where the cohesion is least. If there are air-bubbles in the ice, the capillary fissures have a tendency to strike through them. Ice abounding in air-bubbles presents a mass of less cohesion; and the same pressure will in such a case produce more numerous divisions, a smaller grain, than in ice which is more compact.

To this the conditions in the glacier precisely correspond. In the upper parts the ice appears white through a multitude of bubbles; and there it is small-grained, and the air-bubbles are penetrated by many times their number of capillary fissures. Through these water can reach the cavities of the former, and freeze therein, and the air escape. Thus the number of bubbles diminishes, the ice becomes continually more compact as it moves forward, and hence the grain larger. In the upper parts of the glacier, where the transverse section is larger, the pressure which the individual parts have to sustain is also greater; further down, it is diminished with the transverse section, and hence the new capillary fissures become less numerous.

Hugi made the observation (I confess I have never repeated

* The welding of metals (iron, platinum) is probably quite the same process as the regelation of ice; at least I know of no essential difference. When two badly welded pieces of iron break asunder again, the surface of fracture is also wrinkled. It is likely that accurate comparison of the two processes would throw more light on their nature.

it myself) that, in a glacier which has to follow a sharp curve in the valley, the grain is smaller on the concave side than on the convex one. In the middle the pressure is the usual, normal pressure, on the concave side a maximum, and on the convex a minimum; there the capillary fissures are more numerous than here: therefore this also is concordant.

If these explanations are correct, it is clear why, immediately below the limit of the *névé*, a distinct glacier-grain never occurs in the middle of the surface; for these parts of the ice have never yet been compelled by strong pressure to considerable differential motions. *The greater the pressure, differential motion, and porosity of the ice, the more will it be divided, and the volume of the grain the smaller; with the diminution of the pressure the division diminishes, and the volume of the grain increases.*

This theorem could have been arrived at by deduction; it agrees with the facts known to me, and thereby commends itself to further examination.

That the number of bubbles diminishes in the manner above indicated I will not positively assert. We know the properties of glacier-ice only near the surface. There is a vast interval between this and the knowledge of what goes on within the glacier.

In the badly conducting mass of the glacier the cold of winter can only penetrate to the depth of about 8 metres. In the spring, where the first water penetrates from above into the refrigerated layer it is frozen and occasions an increase of volume (a swelling). But this is very limited; it only affects the uppermost layers, and is confined to a few brief periods.

M. Grad ascribes to the freezing of the infiltrated water in the capillary fissures, not only the enlargement, but also the "crystallographic orientation" of glacier-ice, discovered by Bertin, and afterwards confirmed and generalized by MM. Grad and Dupré. But he seems to me to form no accurate conception of how this is to be produced by the water freezing round the surface of the grains. It might be very difficult to do so. I am inclined rather to look upon the "crystallographic orientation" as a function of the pressure. The ice masses of the lower part of the glacier, in which it has been observed, are not the same which in the upper part did not exhibit the phenomenon; they are those which, during many years, have sustained the mighty pressure of the overlying layers of ice, now melted away. When we consider that, in a body the temperature of which is always near its melting-point, molecular derangements readily take place, and that there is no lack of shocks which go through the mass of ice (such as occurs, for example, in the crash whenever a crevasse opens or closes), the latter notion becomes still more

probable. The phenomenon observed by MM. Bertin, Grad, and Dupré, that in lamellæ cut horizontally out of the lower part of the glacier coloured rings with a black cross are seen when they are viewed in the polarizing microscope, does not necessarily indicate actual crystalline structure; amorphous glass can yield the same phenomenon through strains forced upon it by external pressure. It appears to me that the effect in the glass also would necessarily be permanent, if the violent pressure had operated for many years. Experiments on a small scale, for the purpose of producing by pressure the crystallographic orientation in pieces of ice, gave no result. This is not surprising; for I could not, as many a glacier does, operate with a pressure of 5 cwt. per square inch, or factors of similar magnitude. Already in the 27th volume of the *Philosophical Magazine* Sir John Herschel conjectured a parallel arrangement of the optic axes, but not on grounds corresponding with M. Grad's explanation.

I must not, however, omit to mention that, according to the calculations of an Englishman (Canon Moseley) in the *Philosophical Magazine* for May, 1869, the resistance of the ice to the shearing-forces of the glacier-motion would be too great for the ice to be broken by gravity alone and thus the glacier to move by its own weight. If I rightly understand the experiments of Tyndall and my own on the remoulding of plates of ice, the ice therein and in the glacier is not compelled to shear, but to break by the bending. The shearing, tangential displacements take place along the fissures previously produced by fracture (bending). The very peculiar mechanical conditions of the ice seem to me not to have been sufficiently considered in the calculation. One of these is its extraordinary brittleness even at 0° C.; the same pressure which a mass of ice will sustain in a state of rest for a long time without breaking, breaks it immediately a shock is added. Another is the occurrence of crowds of minute air-bubbles, which must very much diminish the compactness of the ice. In the lower part of a glacier, where air-bubbles are almost absent, we have the old, only half regelated secondary capillary fissures, along which fresh fracture is easier: while in this region the primary fissures are somewhat less numerous, the secondary (which are longer preserved) exhibit a peculiar abundance of plaits. So many factors, unmeasured in their effect, and scarcely known in their mode of operation, are involved in the mechanics of glacier-motion, that the result of a calculation based on the little that is known cannot possibly induce me antecedently to reject the explanation by pressure, with which all the facts of which I have any knowledge, and all that I have seen, agree so perfectly. According to the explanation of glacier-motion given by Mr. Moseley (*Phil. Mag.* Jan. 1863 and Aug. 1869), the total motion must

be, on the average, equal in amount in the upper, middle, and lower parts of the glacier, which is not the case. As in no part of the glacier, certainly not at the lower end, has any (even the least) upward motion ever been observed at certain times, the line which, as a base, remains relatively fixed must, when the temperature falls, lie at the upper end of the glacier; when the temperature rises, at the lowest point. That with rise of temperature the lower end would remain stationary, and drag after it the whole long glacier-tongue (which cannot descend by its own weight), without its being rent transversely into single independent fragments, I cannot conceive. The surface of the glacier undergoes more and greater variations of temperature near its margins than in the centre—now from reflection of heat from the sides of the valley, then from their shade, but especially through the winds (which in the centre arrive with their temperature already approximated to that of the glacier). The margins have in some measure a more continental, the centre a more oceanic climate. Hence one would think that the edges of the glacier would move faster than, or at least as fast as the centre, if variations of temperature effected the motion.

I have not the remotest intention to summarily reject Mr. Moseley's views; but I thought it admissible to state what at present appears to me opposed to them, in order to justify my continuing to hold Professor Tyndall's explanation. Perhaps Mr. Moseley will be able to remove these difficulties and, especially by measurements of the interior temperature of glaciers, give his views a better foundation.

In the *Philosophical Magazine* for March 1869, a "new theory of glacier-motion" is advanced by Mr. Croll, which, as regards the shearing-forces, is free from the difficulties of Professor Tyndall's theory. I must omit a discussion of this theory, because I do not yet clearly understand it. According to it an explanation of the vertical arrangement of the optic axes would be very easy.

2. *Experiments with "killed" Gypsum.*

The theory of plasticity, as given by Forbes, led immediately to experiments with semiliquid masses. At that time the difference between viscous and non-viscous semiliquids was neglected. Both yield to pressure; but while the former extend on being drawn, and contract transversely, so as even to form thin threads, the latter resist change of form by traction or are torn across. In the former the parts are rearranged by traction and by pressure; in the latter, pressure produces a rearrangement, traction either no effect or division. Ice is solid and very brittle; neither by pressure nor by traction are its particles

perceptibly displaced; either their position is quite unaltered, or they are completely separated—the piece breaks. This property is also possessed by glacier-ice. Pressure does not alter the arrangement of its particles, but separates them through fracture at the capillary fissures; and all the ice fragments which are traced out thereby (the glacier-grains) behave now like the molecules of semiliquid masses, which are plastic to pressure. The glacier cannot possibly behave to traction like a “*viscous body*,” because the grains partly are separated from each other and have not, like molecules, a sphere of attraction surrounding them, partly unite by regelation and then constitute an aggregate like a piece of water-ice, brittle.

If with homogeneous semiliquid masses we would make experiments on motion which can be brought into direct relation with what is observed in glaciers, their molecules must be merely displaced by pressure, and not merely displaced but separated by traction. But in bodies of this class known to me the pulling force necessary for separation is very small, less than the weight of each single molecule; and this gives them the property of thin liquidity. We cannot use them for experiments on glacier-motion, because, if a fissure were to ensue, the weight of the particles at its margin would immediately drag them down into it, so that no fissure could remain.

I see no simpler expedient than the following. We must procure a thickly liquid mass, not homogeneous, but consisting of solid corpuscles adherent so that, drawn asunder, they are easily separated, while yet the adhesion is sufficient to neutralize the weight of a small number of corpuscles, but not of a whole mass.

Wet sand, moistened powder, &c. would be such. Through other labours I was first led to “killed” gypsum. “Burnt” gypsum powder is seen under the microscope to be distinctly crystalline. Moistened with water, after a little while it suddenly shows lively motion; the form changes almost momentarily, and other gypsum crystals appear. At the same time the mass stiffens. After a few seconds, a multitude of new, quite fine, needle-shaped crystals make their appearance, which, when not disturbed, unite in beautiful radiate groups; they add to the density of the texture formed by the other, larger crystals, and thereby enhance the solidity of the stiffening mass. The moment of violent motion is probably that at which the crystals of “burnt” gypsum take up water. If now, at the moment the circumcrystallization ensues, fresh water in proper quantity be poured in, and the mass be briskly stirred or moved, the crystals cannot interweave themselves, but will remain single and free; the mass will not stiffen again, and may be kept for

any length of time in a thickly liquid state. Dried by heat, it becomes full of cracks and mealy. Such a gypsum paste the potters and plasterers call "killed."

Killed gypsum therefore consists of rod-like and needle-shaped crystals only loosely adherent by the water in the interspaces; it is a mass of wet powder.

By the addition of more or less water, any degree of thick liquidity can be exhibited. The experiments on the motion of killed gypsum were conducted in the following manner. Out of potter's clay a glacier-valley, or several joined together were prepared, with windings and more and less steep, narrower and wider parts, about 4 feet long; and then the gypsum paste was poured in at the upper part, and this put into flowing motion as slow as was desired by knocking at the base or by raising the upper end. With paste slightly liquid the motion was a simple flowing; the more thickly liquid it was made, or the smoother the valley-sides, the more was the motion a compound of sliding and flowing; and it could easily be employed so thick that the motion exhibited only a sliding of the whole mass along the valley-sides. Corresponding to the conditions in glaciers, a mean was preserved, which effected a motion compounded of sliding and flowing. The fissures produced by non-uniform motion are often as fine as a hair. If the water with which the gypsum is killed be previously coloured darker (for example, blue), these figures will all be still more evident, being filled with the coloured water, while the white gypsum will be only tinged. The whole impression given by these gypsum streams is perfectly that of a glacier. My honoured teachers, Professor Escher von der Linth and Professor Mousson, have viewed some of the experiments. The following are some of the results.

The Middle Wall of Compound Glaciers.—If nearly equal streams of gypsum be caused to flow through two valleys into a third formed by their junction, immediately at the confluence a depression is produced solely by the mechanical force of the motion, but a very little further on a decided central wall-like elevation which soon attains a constant height, and only after a longer course gradually disappears again. Each of the two streams was semicircular in transverse section, about 5 centims. broad, and 2 centims. deep; the united stream was 8 centims. broad. The resulting central longitudinal wall had at its most perfectly formed part a breadth of about 13 millims. and a height of 3 millims. The phenomenon is often very striking, but does not always occur in this manner; it is very much influenced by the angle of junction of the streams, the form of their transverse section, &c.

This immediately suggested the conjecture that a part of the

elevation of many a central moraine of compound glaciers is produced, not by the underlying ice being protected from melting by the *débris*, but in such a purely mechanical manner. The central moraine would then, where the mechanical middle wall is rising, increase in height more quickly than further on, where the mechanical dam is already formed and attains no further elevation.

In the Aar Glacier, for a length of 1200 metres, the whole central-moraine dam is not covered with *débris*, but only crowned therewith; and this extent commences about 400 metres below the confluence—exactly where in the 12000-fold diminished scale of the gypsum experiment a middle wall began to arise. Afterwards the *débris* slide down the side and cover the previously clear declivity. Whether, as in the gypsum stream, it sinks again, I know not. Fig. 2 (Plate VII.) represents the profile of a transverse section through the middle of the Lower-Aar Glacier, 250 metres below the union of the two principal streams—the Lauteraar (L) and the Finsteraar Glacier (F); figure 3, one at about 1000 metres below the point of union; and figure 4, one at 3300 metres further down. A peculiarity of the Aar Glacier is, that (as shown in fig. 3) the moraine-crown is not exactly on the centre of the wall, but is displaced towards the left-hand side of the valley.

Altogether the same as in the Aar Glacier, but much more striking, I found in the Steinlimmi Glacier where it is joined by a powerful arm of the Stein Glacier, which has a precipitous fall between Bocksberg (2640 metres) and Thierbergli (2754). Fig. 7 is a sketch of the moraine of the Steinlimmi Glacier, with its wall clear of *débris*. Fig. 5 shows the profile where the two lateral moraines are not yet quite united, in about the place which coincides with the middle of fig. 7.

As in the gypsum experiments, certain irregularities in the form of the bed will always prevent the formation of a distinct mechanical middle wall; thus it does not appear when the two streams are very unequal in magnitude; hence profiles like the above are by no means to be expected in all compound glaciers. Since my attention was directed to this point, the two glaciers cited are the only ones I have seen of those composed of two nearly equal streams.

It appears to me that it was the observation of such profiles as figures 3 and 5 that misled Charpentier to delineate, as shown in fig. 6, the profile of a moraine as protecting the ice from melting away, and to speak of a "*piédestal des moraines superficielles*," while with respect to lateral moraines he makes no mention of elevation through ablation of the glacier, and was not yet aware of the widening of moraines by the *débris* sliding

down into cavities produced by melting, but only treats it as the effect of the extension of the glacier into a wide valley (*Essai*, § 21, 1841).

Fissures of Displacement.—If across the surface of a stream of killed gypsum a straight line be drawn of another colour, one might have expected that, in consequence of the greater velocity in the centre, it would be drawn out into a continuous curve with its convexity turned towards the lower end of the valley, instead of which we obtain a form like fig. 8. There appear displacement-fissures approximately parallel to the margin, especially numerous near the bank; and on these our line is broken, producing a broken curve. A row of stakes driven into the ice, in a line across the glacier, will not suffice to enlighten us concerning the corresponding condition there; we must have a distinct continuous line. Such a one can easily be made by gathering small dark-coloured stones and laying them close together in a straight line. They form a line which cannot be disturbed by the most violent shower of rain, because, through greater absorption of heat, they immediately sink into the ice. At the end of August, 1869, I, with the aid of my friend Alfred Kleiner, Stud. Med., and Joseph Maria Trösch, chamois-hunter of Maderau, placed such lines in two different places on the Upper Rhone Glacier, and at the beginning of September across the Hüfi Glacier. When, three weeks later, I again visited the localities, all the lines had remained quite entire; but the interval was too short for a perceptible change in the form of the lines to have been produced. Hoping that our lines would last through the winter, and in the summer of 1870 more change of form would be visible, I descended the valley again.

Although I have not yet demonstrated the displacements, yet I believe I have discovered displacement-fissures of the most definite character, especially on the Upper Rhone Glacier. When the bed of a glacier is regular, we know that the principal fissures proceed from the margin obliquely upwards towards the middle. These may often have considerable breadth. They are perpendicular, or nearly perpendicular, to the ice-structure (the blue bands) in the lateral parts. Now, in the Upper Rhone Glacier, there is also a system of fissures which are partly parallel to the structure, and partly cut it at slight angles. This system has hitherto remained unnoticed, because these fissures do not gape; mostly they only offer space sufficient to insert the blade of a pocket-knife, and at first sight may be confounded with the structure. They penetrate the mass of the glacier, and can be traced downward in the deepest principal fissures. They are very regular in their course—as far as we could trace them, always rectilinear—and are continued till they cut the bank at

an oblique angle, or disappear beneath the lateral moraines—which gives lengths of 800 metres and more. There are never two very near together, but they are at nearly uniform distances (perhaps from 10 to 20 feet), like the displacement-fissures in the gypsum stream. We found by many excavations that the ice is perfectly divided in these fissures, while it is easy to cut a piece of white glacier-ice through which blue bands run transversely, and the structure does not destroy the cohesion.

Where the structure cuts these fine fissures, many displacements which have taken place along them are directly demonstrable; and in those which we have found, the fragment nearest the middle was always in advance of that nearer the bank. See fig. 9: the structure-bands *a* and *a*, *b* and *b*, on the two sides of the displacement-fissure *x*, correspond to each other. From different points of the valley-slopes we viewed the glacier under different lights, in order to see the course of these fissures over the whole glacier; but at a little distance they cease to be visible, even under oblique illumination. How difficult it is, on the surface of a glacier, to take the bearings of these and similar objects, he only knows who has experienced it. The enormous dimensions of the surface, the excessive number of great elevations and depressions all resembling one another, the large open fissures one must go round, the attention he must pay to his footsteps—all this renders the survey incredibly difficult. On this account I was obliged to content myself with ascertaining the course of the displacement-fissures on the eastern side of the Upper Rhone Glacier. I afterwards found on other glaciers, especially near irregular banks, similarly situated fine fissures with quite the same character, but could not accurately follow their course. They are met with chiefly where the motion is retarded at the margins more considerably than in a regular valley, as behind projections. Fig. 10 shows their course in the Upper Rhone Glacier: *a* denotes the displacement-fissures, *b* the direction of the structure, *c* the usual open fissures. Figs. 10A and 10B represent on a larger scale, with the correct angles, the parts at A and B in fig. 10. Fig. 9 is also to be supposed in the region of B, fig. 10. The large bay between D and E must occasion great retardation of motion in these parts of the Rhone Glacier.

In the gypsum stream the displacement-fissures, when numerous, may altogether replace the ordinary gaping fissures; in glaciers they appear rather to be a local phenomenon, while the gaping fissures are universal. Whether the system of fine fissures found on the Rhone Glacier is really the analogue of the displacement-fissures in the gypsum stream can only be decided by subsequent observation of the stone lines which we

constructed: their situation on the Rhone Glacier is indicated by thick lines in fig. 10:—one in the region of A; the other on the other side, above.

In a somewhat more complicated experiment with killed gypsum, in which two larger and one smaller streams were brought to confluence, longitudinal lines also were interrupted by displacements. The middle wall of the two larger streams was cut through obliquely by fine fissures at which the gypsum was slightly displaced vertically. In one set of experiments the cracks corresponding to the usual glacier-fissures, which go obliquely upward from the margin toward the centre, were very distinct; but instead of being very open, they were more numerous. In other experiments they appeared to be rendered unnecessary by displacements in the mass. An attempt to photograph all the fine forms must be repeated.

Expansions.—When a glacier, in consequence of entering a wider part of its valley, can expand at its lower end, at the upper part of the widening it throws out longitudinal fissures which, curving to the borders, form at the lower end a radiating, fan-like system. This is readily intelligible. The pressure of the middle portion, to which the ice can now yield, is radial, and carries the ice in a periphery having the end of the glacier for its centre; the fissures are thrown out perpendicular to the direction of motion, and are therefore radiating. Remarkably it is not so in experiments with killed gypsum. There two systems of fine fissures occur at the end, which cut one another at a constant angle of from 69° to 72° . These fine fissures never gape. At them the lower parts are displaced vertically upward. They are nearly similar triangles—one side formed by the border of the out-spread mass, the two others by a fissure of each of the two systems. The system of radiating gaping fissures in the glacier is here replaced by two systems of displacement-fissures which cross one another.

In wet masses so small, adhesion is proportionally strong, and more opposed to divisions than to displacements. We have already above encountered this preference in the killed gypsum for displacements, which appears to me to be the cause of the difference here—though perhaps it has a deeper foundation in the essential nature of the substance. Unfortunately I was unable to investigate other semiliquid non-viscous substances in this relation; nor do I know how far the angle of about 70° , peculiar to killed gypsum, depended on the degree of thick liquidity. In glaciers I have not found any trace of a double system, much as I have sought for it. Fig. 11 shows the course of the fissures at the end of the Rhone Glacier; fig. 12, the displacement-fissures at the expanded end of a gypsum stream.

Differential Motion.—It has already been endeavoured to refer the structure of glacier-ice (the blue and white bands) to surfaces of greatest differential motion. This, like the ice-structure, takes place at surfaces which have the form of spoon-bowls laid one in another, the drawn-out end downward. Such surfaces can easily be obtained. If in the gypsum stream we colour one layer differently, it will be seen, when in motion, to assume this form. The differential motions, however, do not take place in these bands, but cut them, as shown in fig. 8. Professor Tyndall has already called attention to this point. Whether the differential motion is greatest on these spoon-shaped surfaces, or parallel with the sides of the valley—in other words, whether “the direction in which the filaments slide past each other will be obliquely directed toward the middle,” or parallel with the middle line, I have endeavoured to prove by an experiment. I will develop it, because it presents points of connexion in another direction.

A number of lamellar or linear solid corpuscles in a thickly liquid mass remain quietly in their position without any definite arrangement when every part of the mass moves with equal velocity; the lamellæ are in no wise all arranged (as has recently been said) perpendicular to the direction of the flow—for example, in lavas. A definite arrangement is only brought about by differential motion (when some parts move more quickly than others). This is everywhere the case where the mass flows in a fixed bed. There friction diminishes the velocity near the sides of the bed (lava, mud-streams, glaciers). When a lamella lies in a flowing mass, the end of it turned to the more slowly moving side moves more slowly, is kept back relatively to the other; the lamella is turned till it falls into the surface of greatest differential motion; and in this position it remains. Were it by accident to be turned beyond, it would immediately be tipped over, and again be turned in the same direction into the same surface, as at first. All lamellæ in a flowing mass arrange themselves in the surfaces of greatest differential motion.

I cut an immense number of minute chips of wood, of about a square line surface and the thickness of stout paper. These were mixed with the gypsum, and the mass set flowing along the valley-channel, and afterwards dried up solid (in which process, it is true, the form was a little distorted); and now the position of the chips was investigated. Fig. 13 shows the arrangement in a longitudinal section through the middle line; therefore the surfaces of greatest differential motion are parallel to the sides of the bed. In the middle of the higher parts, where generally the differential motions are much smaller, no arrangement had entered. If the other view were correct, the

longitudinal section would have given the form shown in fig. 14.

The "sliding of the filaments towards the middle" is somewhat plausible if we consider that at the margins of the glacier, where the motion is much slower, a heaping up of masses, and in consequence a movement of them toward the middle must take place. This, however, is the case only at the uppermost part of the glacier, where the wide channel of the *névé* opens into the narrower glacier-valley. There the chief mass moves toward the middle, and at the margins only that is driven after which has plenty of room. In the glacier itself, as the undisturbed course of the central and lateral moraines shows, it is otherwise. The glacier behaves as a number of half-cylinders lying one within another, the inner sliding more quickly down the valley than the outer; there is no trace of heaping-up at the margins. The only question is, are these cylinders infinitely thin? that is, does the velocity increase continuously toward the middle? or does each set of infinitely thin ones, which do not admit of much displacement of one over another, form a thicker cylinder? that is, does the velocity increase *per saltus*? do displacements take place as in the gypsum stream (fig. 8)? If I have correctly indicated the displacement-fissures in the Rhone Glacier, they are, in this comparison, the divisions between two thicker half-cylinders.

When, analogous the valleys of the Rhone, the Trift, and many other glaciers, a very steep place is made in the valley, the gypsum stream breaks down over it exactly as does a glacier, in the form of stairs. At the foot of the fall the terraces gradually become small transversely running arcuate elevations, and at length vanish altogether: in glaciers, as is well known, they leave the dirt-bands behind.

If small pieces of wood are placed upright in the gypsum stream, they very soon incline toward the bottom of the valley—a proof that the velocity diminishes downwards.

That in a winding valley the maximum of motion approaches the concave side, that the glacier scarcely reacts on small lateral glens in its valley, that it presses aside or overpowers small lateral affluents, &c., all these can be very beautifully demonstrated in the stream of killed gypsum. Of course the forms of the surface are never those of the glacier, because there is no ablation, and at the progressing lower end of the gypsum stream the motion is a rolling; while it is not so in the glacier, on account of the ablation, or this rolling only occurs, quite sub-ordinately, in winter, if even then.

If killed gypsum is caused to move slowly from a shallow cavity into two valleys in opposite directions, at the summit

of the cavity a splendid system of transverse fissures is obtained, exactly as they are formed in *névé-plateaux*, which are the boundaries of glaciers. This system of fissures can never be replaced by displacements.

With all this a variety of thoughts, leading to further experiments, connect themselves—for instance, about an arrangement of semiliquid masses in a systematic series according to mechanical principles, &c. Perhaps I shall subsequently resume the subject. The experiments, still very incomplete, could not be further continued on this occasion; for as in a mountain-stream the rubble stones move more slowly than the water, so our labours are always outstripped by time.

3. *On Structure.*

In order to prove that the structure of glacier-ice (the blue bands) is not derived from the stratification of the snow, Professor Tyndall searched for places where the structure and the stratification might be seen together, and after long searching he found them. On the Upper Rhone Glacier, in the summer of 1869 we had continual opportunities of repeating and confirming his observation. In almost all the fissures the snow-bedding was horizontal; and almost perpendicularly cutting it the structure was visible; so that we could have wished Professor Tyndall had first sought for it here; he would have sooner found what he wanted to see. The same was distinctly to be seen abundantly on the Upper Trift Glacier, and even on the little Glärnisch Glacier.

A second proof of the difference between stratification and the structure, and partly also a support for Tyndall's explanation of the origin of the structure, is the following:—I thought to myself, "If a glacier with distinctly developed structure (longitudinal, or much-drawn-out transverse structure) has afterwards to make a fall, the new transverse structure originating at its foot must cut the already present longitudinal structure. Were the structure a stratification, it could not cut itself." The Rhone Glacier, in its upper part, has already the ordinary structure; we therefore sought at the foot of the fall, in the "structure-mill," for crossed structure. So regularly, however, was the fact not as I had thought, that the masses in the fall tumble into considerable disorder, are many times twisted and dislocated; the old structure could not be traced throughout its course, but was only partially distinct; it was often not clear to me whether I had to do with the new or the old. However, after long search and diligent cutting up of the upper white crust (it was a splendid day at the end of summer), we found an immense number of places, scattered over the whole

breadth of the glacier, in which crossed structure was distinctly visible—for example, in the fine ice full of white bubbles a blue band one inch broad, cut at an angle of 40° by another of nearly equal breadth, and others similar, even a complete system of bands which crossed one another, as distinct as one could wish. Further down the “structure-mill” the new structure was so predominantly complete, that traces of the old were not easy to find.

In many other glaciers crossed structure is probably still more readily to be found; and every thing will be more regular when the fall is not so wild and the structure is better developed in the upper part than in the Rhone Glacier.

The bounding surface between bubble-free (blue) ice-band and bubbly (white) ice can be readily laid bare in a hand specimen by a carefully directed stroke. It immediately strikes one that it is cylindrically grooved vertically. At the first glance the following explanation obtrudes itself:—The paths which, in the formation of the blue band, the retreating air-bubbles have taken are preserved, filled with ice, reminding one of kernels in stone; and in isolated cases the air-bubble does not urge its way quite to the top, it stays at the end of the ice-kernel which marks its unfinished path. How much this observation supports Professor Tyndall’s explanation of the origin of the blue bands in the white ice is obvious. I made it first on the Upper, then on the Lower Rhone Glacier, and afterwards found it confirmed in other glaciers. Fig. 15 (Plate VII.) represents a piece of white glacier-ice in which the bounding surface of one of the blue bands is laid bare.

When the minute displacement-fissures are no longer operative as such, the water in them at length freezes to a bubble-free narrow ice-vein of at most 1 centim. thickness. This is readily distinguished from the blue bands by its regular course; it has a somewhat different aspect; in addition to which it usually cuts the structure at a slight angle, as is frequently to be seen in the walls of the normal open fissures (in fig. 16, *a* is the ice-vein). On the left side of the Rhone Glacier these glacier-veins are found pretty numerous both immediately above and below the fall. I have never seen on their bounding surface a cylindrical structure. This is at once explained by their different manner of formation.

Far more suitable than any other glacier for the study of the origin of the dirt-bands and structure is the Trift Glacier, which on the Triftlimmi is connected as a névé cap with the Rhone Glacier, but descends in the opposite direction, into the valleys to the north. Its principal fall is incomparably more magnificent and wild than that of the latter. Thence in a length of

2000 metres, with a breadth of 1100, the glacier has a descent of only 115 metres; this is followed by steeper, almost precipitous portions, extending 1800 metres. From the Clubhütte (which stands close above and beside the glacier) one commands a view of the whole, and can perfectly gather immediately from Nature the explanation given by Professors Forbes and Tyndall of the origin of the dirt-bands. The structure is so distinct that it can be accurately followed throughout its course. Never have I enjoyed so magnificent a mountain-scene as this; little known, it surpasses in beauty all the larger glaciers I have seen.

4. *Névé-snow.*

On the 25th August, 1869, we ascended, under a cloudless sky, the Galenstock; and on the 28th we walked up the whole Rhone Glacier, and over the Triftlimmi. There, all over the surface, the snow was finely ribbed as I do not remember having ever seen it before. In two directions, at an angle of about 60° , the snow was finely furrowed, and thereby divided at the surface into little rhombs, the longer diagonal of which (perhaps 2 or 3 centims.) had in every case an east and west direction. Fig. 17 shows their form. The wind often produces something similar in the névé-snow; but the immense regularity and universal distribution were nevertheless peculiar.

LXIV. *On Secondary Planets in Small Orbits.*

By DANIEL VAUGHAN, Esq.*

FROM the theories of the tides and of the figure of the earth, it appears legitimate to pass to kindred researches on the extent in which the close proximity to a great central orb may affect the form of a satellite, the gravity at its surface, or its capability of maintaining the planetary structure. On this subject I partially treated in communications for the *Philosophical Magazine* about ten years ago. But it seems desirable at the present time to extend the investigations which I then presented, as they will be found to have an important bearing on certain questions of scientific interest, and the basis on which they rest can be rendered more secure. In supposing that the same side of a satellite is always turned towards its primary, there might appear some dealing in uncertainty; for the arrangement which our moon and other secondary planets exhibit could not be assumed as the result of a general law as long as it was ascribed to special causes supposed to operate in very remote ages. The assumption might be more justifiable if,

* Communicated by the Author.

in accordance with modern doctrines, the ultimate effects of tides on the rotation of a satellite were regarded as the cause of the synchronism of its orbital and rotatory movements; yet there would seem to be too great a demand on hypotheses in claiming for every minor world the possession of seas and the temperature required for their fluidity.

A more certain, though perhaps a more slow cause of the arrangement in question, will be revealed on considering how a solid satellite, consigned to a small orbit and rotating in such a manner as to present its opposite sides alternately to the primary, must have its form perpetually changed by the attraction of the latter body; for these changes (even when they proceed from oscillations of the internal matter with unimpaired elasticity) would have the same effect as tides in altering the rotation until it kept pace with the periodical revolution. This I have shown in the *Philosophical Magazine* for December 1861; but it may be proved more easily by the aid of the modern doctrine of the definite relation between heat and motion. In the case of the supposed secondary planet, with an incessant change in the gravity at its surface and in the pressure on its internal parts, the resulting oscillations must be accompanied by the production of heat; and this being necessarily generated at the expense of motion, the rotation would evidently change until it became adapted for keeping the same side ever confronting the primary. The want of coincidence of the planes of orbital and diurnal motion and the eccentricity of the orbits would also be corrected by the oscillations and the calorific action resulting from them; and the surface of the satellite would thus constantly assume a more tranquil condition, as the disturbance from the central body varied little either in intensity or in direction.

For producing and maintaining the arrangement so favourable to repose, the causes alluded to are most effective when a large secondary planet is in the closest proximity to its primary, and those extreme cases where stability is scarcely possible may be investigated with the least dependence on hypothesis. Such cases are also recommended for consideration by their connexion with certain celestial phenomena. For maintaining the planetary structure of two large satellites in the region occupied by Saturn's rings, so great a density would be required, that the condition of the mysterious annular appendage must be ascribed to its proximity to the primary. While also the resistance of a space-pervading medium may be expected to consign to an orbit too small for stability a member of some of the numerous planetary systems of the universe, an exact investigation will show that the great dismemberment must take place,

not gradually, but in a paroxysmal manner. These results, together with some facts respecting the light of meteors, furnished the chief grounds on which I have hitherto maintained that temporary stars are great meteoric displays attending the dilapidation of a world, its fragments sweeping through the atmosphere of a greater central orb. More positive evidence is afforded by observations made on the star which displayed its ephemeral brilliancy in the constellation of Corona in 1866.

That hydrogen is the main constituent of the highest part of the solar atmosphere was first inferred by Dr. Stoney from theoretical principles, but has been proved more conclusively by the observations of late years. As the same light gas is now known to exist in most of the great orbs of space, it must form the chief part of their external envelopes, in obedience to the law which gives fluids a position depending on their density, and which, from the great rarefaction and feeble expansibility of the uppermost atmospheric strata, must overrule the tendency which the power of diffusion exerts for producing a uniform mixture of all æriform collections of matter. From their position in the outward envelopes of all great celestial bodies, the most rare and attenuated elements must act a most conspicuous part in such great meteoric displays as may be expected to occur when a primary or a secondary planet, gradually introduced into too small an orbit, undergoes either a total or a partial dismemberment, and sends its innumerable fragments through the atmosphere of the central orb. As the fragments sweeping through the upper atmospheric stratum of the latter body would escape immediate precipitation to its surface, and continue the production of heat and light during many revolutions, and as large meteors must require some time to partake of the high temperature which they impart to the resisting medium, it may be reasonably expected that the peculiar characters of hydrogen and of other rare gases would be impressed on the vast flood of light sent forth during these exhibitions of meteoric brilliancy.

This explanation accords with the fact that the temporary star of 1866 exhibited, not only the ordinary spectrum of sidereal bodies, but also another, consisting of four bright lines, two of which indicated the presence of hydrogen gas in a high state of incandescence. The eminent observer to whom science is indebted for this discovery, ascribes the phenomenon to effects of burning hydrogen; but the cause would certainly be inadequate to call forth the wonderful brilliancy of the star, even though the gases participating in the combustion could meet in the exact proportions required for chemical union, and no inert elements reduced the temperature by sharing the heat

which they took no part in producing; but if surrounding a star equal in mass to our sun, an envelope of hydrogen traversed by meteors would acquire from pressure alone a temperature many thousand times as great as that arising from its combination with oxygen. The disproportion in the efficiency of the two causes will appear still greater when the slowness with which large collections of different gases can mix, so as to afford scope for the play of affinity, is contrasted with the rapidity with which a host of meteors in the supposed case would make their terrific influence felt throughout an extensive region of a solar or a planetary atmosphere.

In investigating their form and stability in small orbits, I shall regard the satellites as fluid; for when their size is considerable the resistance of solid matter cannot occasion any wide departure from a figure of equilibrium. The mechanical theory of the resistance of solids shows that passive strength increases in a less rapid ratio than weight, and that large structures are weaker in proportion to their size than smaller ones. But in bodies as large as worlds the restraint from solidity is far less than that given by theoretical estimates, as a strain extending over many hundred miles must fall on some localities in an undue proportion; and in the planetary structure rents would be gradually produced, its several parts yielding in succession to a force which, if duly distributed as theory supposes, might be successfully resisted by the solid mass. Accordingly, in questions relating to the form of a large satellite, the gravity at its surface and the radius of the smallest circular orbit which it can describe, no serious error can arise from adopting the hypothesis of fluidity. A source of greater discrepancy in the results for different bodies would be found in the heterogeneous character of their materials, and in the variable rate at which density might decrease from the centre of each to its surface. But the maximum extent to which this cause can affect the result of our inquiries may be determined by selecting two extreme cases, in one of which the satellite is regarded as homogeneous, while in the other the matter at the centre is supposed to be infinitely dense in comparison with that by which it is surrounded. A similar course has been adopted by writers on the figure of the earth.

I shall take up first the case in which the central matter is so exceedingly dense that it may be supposed to monopolize the whole of the attractive energy, and accordingly that gravity at every part of the surface of the body is directed to the centre. Let D be the distance between the centres of the primary and of the satellite, M and m the measures of the attractions of the two bodies at the distance k , and x , y , and z the rectangular coordi-

ordinates of any point on the surface of the satellite, the centre being taken as the origin of the three planes of reference, supposed to intersect in the axis of rotation, the radius of the orbit, and the line of orbital motion.

Let A, B, and C be the maximum values of x , y , and z , or the major, mean, and minor semiaxes of the figure of equilibrium. The attractive force of the satellite at the given point is equal to $\frac{mk^2}{x^2+y^2+z^2}$; and this may be resolved into three components parallel to each axis and expressed by

$$\frac{mk^2x}{(x^2+y^2+z^2)^{\frac{3}{2}}}, \quad \frac{mk^2y}{(x^2+y^2+z^2)^{\frac{3}{2}}}, \quad \frac{mk^2z}{(x^2+y^2+z^2)^{\frac{3}{2}}}. \quad (1)$$

The attraction of the primary on matter at the same point has the value $\frac{Mk^2}{D^2-2Dx+x^2+y^2+z^2}$; and the components of the disturbing force it occasions will be

$$-\frac{2Mk^2x}{D^3}, \quad \frac{Mk^2y}{D^3}, \quad \frac{Mk^2z}{D^3}, \quad . \quad . \quad . \quad (2)$$

the squares and higher powers of x , y , and z being omitted. If t denote the time, and v the velocity of rotation at the given point, the centrifugal force will be

$$\frac{v^2}{\sqrt{x^2+y^2}}, \quad \text{or} \quad \frac{4\pi^2\sqrt{x^2+y^2}}{l^2}.$$

As the rotation is supposed to take place in the same time as the orbital revolution, l^2 will (from the doctrine of central forces) be equal to $\frac{4\pi^2D^3}{k^2M}$, and accordingly the centrifugal force will be expressed by

$$\frac{Mk^2\sqrt{x^2+y^2}}{D^3},$$

while its components in the direction of the three axes will be

$$-\frac{Mk^2x}{D^3}, \quad -\frac{Mk^2y}{D^3}, \quad 0. \quad . \quad . \quad . \quad (3)$$

If X, Y, and Z represent the sum of the components parallel to each axis,

$$\left. \begin{aligned} X &= \frac{mk^2x}{(x^2+y^2+z^2)^{\frac{3}{2}}} = \frac{3Mk^2x}{D^3}, \\ Y &= \frac{mk^2y}{(x^2+y^2+z^2)^{\frac{3}{2}}}, \\ Z &= \frac{mk^2z}{(x^2+y^2+z^2)^{\frac{3}{2}}} + \frac{Mk^2z}{D^3}. \end{aligned} \right\} . \quad . \quad . \quad (4)$$

Now, from the principle of equilibrium in fluid masses,

$$Xdx + Ydy + Zdz = 0.$$

Substituting the above values for X, Y, and Z, and integrating, there results

$$N - \frac{mk^2}{(x^2 + y^2 + z^2)^{\frac{1}{2}}} - \frac{3Mk^2x^2}{2D^3} + \frac{Mk^2z^2}{2D^3} = 0, \quad \dots \quad (5)$$

the equation of the surface of the satellite N being the arbitrary constant required in integration.

To find the relation between the principal axes, it is necessary to deduce from the last equation an expression for the maximum value of each ordinate, the other two being successively made equal to 0. We thus obtain

$$\frac{mk^2}{A} + \frac{3Mk^2A^2}{2D^3} = N, \quad \dots \quad (6)$$

$$\frac{mk^2}{B} = N, \quad \dots \quad (7)$$

$$\frac{mk^2}{C} - \frac{Mk^2C^2}{2D^3} = N. \quad \dots \quad (8)$$

Subtracting (6) and (8) successively from (7), we find

$$\frac{mk^2(A-B)}{AB} = \frac{3Mk^2A^2}{2D^3}, \text{ or } A-B = \frac{3MA^3B}{2mD^3}, \quad \dots \quad (9)$$

$$\frac{mk^2(B-C)}{BC} = \frac{Mk^2C^2}{2D^3}, \text{ or } B-C = \frac{MC^3B}{2mD^3}; \quad \dots \quad (10)$$

and adding (9) and (10),

$$A-C = \frac{MB(3A^3 + C^3)}{2mD^3}. \quad \dots \quad (11)$$

From a comparison of the last three equations, it appears that when the extraneous force and the consequent deviation from a sphere is small, the ellipticities of the principal sections of the satellite would be nearly in the ratio of four, three, and one.

The greatest degree in which polar compression could be exhibited in such cases would evidently be reached when gravity was entirely annulled at the part of the surface in conjunction with the primary, so that

$$\frac{mk^2}{A^2} - \frac{3Mk^2A}{D^3} = 0.$$

To find, therefore, the greatest disproportion which can occur between A and B, let $\frac{mk^2}{2A}$ be substituted for $\frac{3Mk^2A^2}{2D^3}$, and $\frac{mk^2}{B}$

for N in equation (6); there results

$$\frac{3mk^2}{2A} = \frac{mk^2}{B}, \text{ whence } B = \frac{2}{3}A. \quad . \quad . \quad . \quad . \quad . \quad (12)$$

From equations (8) and (12) it will also appear, after some reductions, that

$$\frac{C}{B} = 1 - \frac{C^3}{CA^3} \text{ or } \frac{C}{B} = 1 - \frac{4C^3}{81B^3}, \quad . \quad . \quad . \quad (13)$$

from which C may be found equal to $\cdot 9576 B$ or to $\cdot 6384 A$. Though not a true ellipsoid, the satellite may, without much error, be considered equal in volume to $\frac{4\pi ABC}{3}$ or $\frac{4\pi A^3}{3} \times 4256$; and if ρ denote its mean density, and g the attractive force of a unit of matter at the distance k , then

$$m = \frac{4\pi\rho g A^3}{3} \times 4256.$$

Regarding the primary as an exact sphere, its radius being r , and its density being taken as unity, M (the measure of its attraction) will be given by the formula

$$M = \frac{4\pi g r^3}{3}.$$

These values of M and m being substituted for them in equation (12), there is found, on reducing,

$$\frac{D^3}{r^3} = \frac{7\cdot 049}{\rho} \text{ or } D = \frac{1\cdot 917 r}{\sqrt[3]{\rho}}. \quad . \quad . \quad . \quad . \quad (14)$$

This determines the size of the smallest orbit in which the satellite could preserve its integrity. If revolving a small distance beyond the surface of the great central orb, it could not hold its parts together unless its mean density were more than seven times that of the primary. To the constitution which I have assigned to the satellite many comets seem to approximate; and from the degree of compression which these bodies exhibit, or from their power to resist the dismembering action of the sun at certain distances from him, we may hope to gain some information of their masses and densities by means of formulæ similar to those I have deduced. But the investigation for comets could not have so well-defined a basis, nor lead to very certain results, as the shape which their attenuated matter assumes depends not on gravity alone, but on forces which seem removed from the domain of scientific inquiry.

For some of the questions involved in the case of a homogeneous satellite, an approximate solution was given in my communications to the *Philosophical Magazine* for December 1860

and April 1861. For a more complete and accurate investigation some of the formulæ which I then employed will be found necessary ; and these I will briefly deduce. While retaining the same notation as in the preceding case, I shall put P , Q , and R for the force of the satellite's attraction at the extremities of the axes, the major axis ranging with the centre of the primary. That the figure of equilibrium is an ellipsoid will be rendered evident by a comparison of the results to which this hypothesis leads. Regarding the body as an ellipsoid, its attraction at the point of the surface represented by x , y , and z will, by a well-known theorem, have the following components in directions parallel to each axis :

$$\frac{Px}{A}, \quad \frac{Qy}{B}, \quad \frac{Rz}{C}. \quad . \quad . \quad . \quad . \quad . \quad (15)$$

From formula (2) the components of the primary's disturbance at the same point will be

$$-2 \frac{Mk^2x}{D^3}, \quad \frac{Mk^2y}{D^3}, \quad \frac{Mk^2z}{D^3},$$

and the components of centrifugal force will be, according to formula (3),

$$-\frac{Mk^2x}{D^3}, \quad -\frac{Mk^2y}{D^3}, \quad 0.$$

Calling the sum of the forces, in the direction of each axis, X , Y , and Z ,

$$X = \frac{Px}{A} - \frac{3Mk^2x}{D^3}, \quad Y = \frac{Qy}{B}, \quad \text{and} \quad Z = \frac{Rz}{C} + \frac{Mk^2z}{D^3}. \quad . \quad (16)$$

In the equation of equilibrium for a fluid mass,

$$Xdx + Ydy + Zdz = 0;$$

substituting the above values for X , Y , and Z , and integrating, there results

$$\left(\frac{P}{A} - \frac{3Mk^2}{D^3}\right)x^2 + Qy^2 + \left(\frac{R}{C} + \frac{Mk^2}{D^3}\right)z^2 = S. \quad . \quad (17)$$

This being the equation of the surface of an ellipsoid, the result justifies the course which has been pursued in assuming this figure as the one to which a homogeneous fluid satellite conforms. When z and y are both equal to nothing, x attains its maximum value and becomes equal to A , and the last equation is reduced to

$$PA - \frac{3Mk^2A^2}{D^3} = S. \quad . \quad . \quad . \quad . \quad (18)$$

The maximum value of y and of z being likewise determined by

putting the other ordinates successively equal to nothing, there is obtained from (17),

$$BQ = S, \text{ and } RC + \frac{Mk^2C^2}{D^3} = S = BQ; \dots (19)$$

whence

$$PA - \frac{3Mk^2A^2}{D^3} = RC + \frac{Mk^2C^2}{D^3},$$

and

$$PA - RC = \frac{Mk^2}{D^3} (3A^2 + C^2). \dots (20)$$

Putting for M its value $\frac{4\pi r^3 g}{3}$, r being the radius of the primary supposed to be a sphere, and g the attractive force of a unit of matter at the distance k , the last equation becomes

$$PA - RC = \frac{r^3}{D^3} \times \frac{4\pi k^2 g}{3} (3A^2 + C^2). \dots (21)$$

The ellipticity at a given distance from the primary may be found by the foregoing equations; but my main object is to determine the greatest ellipticity which the body could exhibit, and the radius of the smallest circular orbit in which it could maintain its integrity. Now equilibrium ceases to be possible on the surface of the satellite, not when the disturbance of the central orb wholly neutralizes gravity in the direction of the axis A, but when it becomes too great to serve as a counterpoise to the forces acting in the direction of A and C. This disturbance is proportional to $\frac{r^3}{D^3}$; and accordingly the maximum value which, for different degrees of elongation, must be given by the last equation to $\frac{r}{D}$, will enable us to express, in terms of the radius of the primary, the radius of the smallest circle which the satellite can describe. In the *Philosophical Magazine* for December 1860 I made, on this principle, an approximate estimate of the size of the smallest orbit, regarding the satellite as a prolate spheroid; but for strict accuracy in calculating this item, and also the ratio of the principal axes of the body, it is necessary to have recourse to elliptic integrals.

For the sake of brevity, I shall avail myself of the results of the investigation which Legendre has given for the attraction of a homogeneous ellipsoid, in his *Exercices de Calcul Intégral*, vol. ii. p. 523. His formulæ, with some modifications in the notation, and taking the density as unity, give

$$\left. \begin{aligned} P &= \frac{4\pi k^2 g A^2 BC}{n^3 c^2} [F(c \cdot \phi) - E(c \cdot \phi)], \\ Q &= \frac{4\pi k^2 g AB^2 C}{n^3 b^2 c^2} \left[E(c \cdot \phi) - \frac{c^2 \sin \phi \cos \phi}{\Delta} - b^2 F(c \cdot \phi) \right], \\ R &= \frac{4\pi k^2 g ABC^2}{n^3 b^2} [\Delta \tan \phi - E(c \cdot \phi)], \end{aligned} \right\} (22)$$

in which the modulus c^2 is equal to $\frac{A^2 - B^2}{A^2 - C^2}$, the limits of ϕ are

from 0 to $\cos^{-1} \phi = \frac{C}{A}$, while n is put for $\sqrt{A^2 - C^2}$, b for $\sqrt{1 - c^2}$, and Δ for $\sqrt{1 - c^2 \sin^2 \phi}$. If the above values of P and R be substituted in equation (21), there will be obtained, on dividing by $4\pi k^2 g$,

$$\begin{aligned} & \frac{A^3 BC}{n^3 c^2} [F(c \cdot \phi) - E(c \cdot \phi)] - \frac{ABC^3}{n^3 b^2} [\Delta \tan \phi - E(c \cdot \phi)] \\ &= \frac{r^3}{D^3} \left(A^2 + \frac{C^2}{3} \right). \quad \dots \dots \dots (23) \end{aligned}$$

Let the ratio of the principal axes be now expressed in terms of the modulus c^2 and the amplitude ϕ . As $\cos \phi$ is equal to $\frac{C}{A}$,

and c^2 to $\frac{A^2 - B^2}{A^2 - C^2}$, it may be readily found that n^3 is equal to $A^3 \sin^3 \phi$, and B equal to $\sqrt{1 - c^2 \sin^2 \phi}$ or to Δ . Substituting these values for B , C , and n^3 in the last equation and dividing by A^2 , we obtain

$$\begin{aligned} & \frac{\Delta \cos \phi}{c^2 \sin^3 \phi} [F(c \cdot \phi) - E(c \cdot \phi)] - \frac{\Delta \cos^3 \phi}{b^2 \sin^3 \phi} [\Delta \tan \phi - E(c \cdot \phi)] \\ &= \frac{r^3}{D^3} \left(1 + \frac{1}{3} \cos^2 \phi \right). \quad \dots \dots \dots (24) \end{aligned}$$

A substitution of the values of R and Q in equation (19) will, by a like process, lead to the following:

$$\begin{aligned} & \frac{\Delta^3 \cos \phi}{b^2 c^2 \sin^3 \phi} \left[E(c \cdot \phi) - \frac{c^2 \sin \phi \cos \phi}{\Delta} - b^2 F(c \cdot \phi) \right] = \frac{r^3}{3D^3} \cos^2 \phi \\ & + \frac{\Delta \cos^3 \phi}{b^2 \sin^3 \phi} [\Delta \tan \phi - E(c \cdot \phi)]. \quad \dots \dots \dots (25) \end{aligned}$$

For finding the greatest extent to which the satellite could have its form elongated and its orbit reduced, I have computed the values of $\frac{r^3}{D^3}$, and of the relation between the forces along the principal axes for different moduli and amplitudes, the latter

being supposed to increase from 58° by uniform increments. In taking c equal to $\sin \theta$, θ in the case in question cannot differ much from 90° , this being its value for a prolate spheroid. I have therefore deemed it best to assume for θ the constant value of 80° until the results exhibit a maximum value for $\frac{r^3}{D^3}$; and then we may determine the extent to which the arc must be altered in order to fulfil the other conditions. Putting P' , Q' , and R' for P , Q , and R divided by $4\pi k^2 g A^2$, the relation between these quantities, and also the values of $\frac{r^3}{D^3}$ for different amplitudes and moduli, will be as follows:—

θ .	ϕ .	P'A.	Q'B.	R'C.	$\frac{r^3}{D^3}$.
80	58	·1883131	·1201880	·1163499	·0659336
80	59	·1830151	·1147750	·1107895	·0663581
80	60	·1776240	·1093840	·1054360	·0666343
80	61	·1721416	·1040776	·0998855	·0669875
80	62	·1665670	·0985860	·0944249	·0672047
80	63	·1609083	·0935796	·0890777	·0671888
80	64	·1550663	·0884503	·0838432	·0669370
80	65	·1493456	·0834122	·0786740	·0667006
80	66	·1434507	·0784623	·0735980	·0662014
79	62	·1674540	·1000739	·0948294	·0676545
78	62	·1684030	·1014850	·0952608	·0681364

From the above Table it appears that $\frac{r^3}{D^3}$ reaches its highest limit when ϕ is 60 degrees; but to fulfil with this value equation (25), θ must be reduced to 79 degrees. From the amplitude and modulus thus obtained it may be easily found that

$$B = \cdot 4988 A, \quad C = \cdot 4695 A, \quad \text{and} \quad \frac{D}{r} = 2\cdot 4547.$$

It thus appears that a homogeneous fluid satellite as dense as its primary would be unstable, even in a circular orbit, if the radius of the latter were less than 2·4547 times that of the central orb, and that, when near the verge of instability, the principal axes would be nearly in the ratio of 1000, 499, and 469. At the point of its surface nearest to the primary the attraction would be almost equal to half that of a sphere with a diameter equal to the major axis, and it would be reduced about 40 per cent. by the disturbing force of the central body.

When the primary and secondary are unequally dense, the formula for the least distance between them must be modified by regarding r as the radius of a sphere equal to the central orb in mass and to the satellite in density. Now r is equal to $r' \sqrt[3]{\rho}$,

r being the actual radius of the primary, and ρ its density divided by that of the satellite; so that the radius of the smallest orbit for the latter will be $2.4547r\sqrt[3]{\rho}$. On comparing the results in the last paragraph with (12), (13), and (14), it will be seen that the heterogeneous composition of a planet would be attended with a far less change in the results in the present investigations than in those usually given for the figure of worlds, supposing the deviation from a true sphere to arise from the sole action of centrifugal force.

Cincinnati, March 24, 1871.

LXV. *On Double Refraction.* By the Hon. J. W. STRUTT,
Fellow of Trinity College, Cambridge*.

IN a former paper† I have shown that, of the various hypotheses which might be made to explain the diminished velocity of light in transparent matter, only one can be reconciled with the observed laws regulating the intensity of polarized light scattered in different directions from an assemblage of particles whose diameters do not exceed a small fraction of the wavelength. We are forced to suppose that the difference between media which is the cause of refraction is a dynamical and not a statical difference, that the rigidity or force with which the æther resists distortion is absolutely invariable. In this view there is nothing novel. Fresnel distinctly adopts it in the investigation of his celebrated formulæ for the intensities of reflected light; and, what is more important, Green's rigorous mechanical theory of reflections‡ is based on the same assumption. Cauchy also, to whom much of the credit really due to Green has been transferred, starts from the principle of continuity of movement, which asserts that in the passage from one medium to another there is no break in the continuity of the values, either of the displacements or of their differential coefficients. I believe that Cauchy has nowhere explained the ground or significance of his principle; but it is easy to see that to assume the continuity of strain is equivalent to asserting a complete continuity of statical properties, so that, as has been pointed out by Haughton§, Cauchy's theory is essentially the same as Green's.

On the other hand, MacCullagh and Neumann have founded their investigations of reflection on the hypothesis that the difference between media is statical and not dynamical. There is,

* Communicated by the Author.

† Phil. Mag. for June 1871, p. 447.

‡ Camb. Phil. Trans. 1838, or Green's 'Math. Papers.'

§ Phil. Mag. S. 4. vol. vi. p. 81.

however, no difficulty in showing that their hypothesis is as inconsistent with the phenomena of regular reflection as it is with those of diffraction from small particles, as I may perhaps explain in detail on another occasion. But there is one argument urged by them against the rival view which deserves the greatest attention. How, they ask, can double refraction be accounted for if the elastic forces brought into action by a given deformation of the æther are the same in all cases? It is well known that all the theories of double refraction hitherto given by Fresnel and his followers assume expressly that within a doubly refracting medium the elasticity varies in different directions. How is it possible, in investigating the laws of reflection from the surface of isotropic media, to suppose that the statical condition of the æther is invariable, and then, when we come to double refraction, to turn round and say that in them the æther has a rigidity dependent on the direction of displacement? I am not surprised at the importance attached by MacCullagh and Neumann to this objection. Fresnel and Green's investigations of reflection are indeed absolutely inconsistent with the received views as to the cause of double refraction. We find ourselves then in this position: either we must give up Green's theory of reflection, which is the only one hitherto proposed, or easily conceivable, capable of meeting the facts of the case; or else we must abandon the ideas of Fresnel as to the mechanical cause of double refraction.

MacCullagh and Neumann were consistent, though, as I believe, consistently wrong. They rejected the hypothesis of a constant rigidity and variable density as incompatible with the existence of double refraction. How, indeed, conceive a density different in different directions?

Fresnel and Green were inconsistent. The latter has given two rigorous theories of double refraction* which differ from one another in important points, but agree in this, that neither of them can be reconciled with his explanation of reflection; for both assume that the forces which resist displacement within a crystal vary according to the direction of displacement. Precisely the same remark applies to the investigations of Cauchy.

It will readily be anticipated that, having the strongest grounds for believing that the rigidity of the æther is constant whether it be free as in vacuum or entangled with the molecules of matter, I adopt the latter of the two alternatives already mentioned, and look in another direction for the explanation of double refraction. In taking a step which may seem retrograde, I would remark that we are not abandoning a theory in itself very complete or satisfactory. Fresnel's explanation of double refraction will always

* Camb. Phil. Trans. 1837. Green's 'Math. Papers.'

be considered worthy of his great genius; but it is well known that as a rigorous mechanical theory it will not bear criticism. Nor do the attempts that have been made to improve upon it carry the mark of truth. On this point I refer to the excellent report on double refraction by Professor Stokes in the British Association's Report for 1862, and will only say that the analogy between the vibrations of the æther and those which may take place in solids, so striking as long as we confine ourselves to ordinary media, seems to break down when we pass on to consider the case of crystals. For Green has shown that the elasticity of a crystallized medium depends in general on twenty-one constants*, while the phenomena of double refraction in biaxial crystals involve only six. It is true that, by assumptions more or less arbitrary, the redundant constants may be got rid of, and the result manipulated so as to agree very well with observation; but no one, I suppose, would consider a theory arrived at in such a manner altogether satisfactory. At any rate this is not the opinion of Professor Stokes, who says that in his belief the true theory of double refraction is yet to be found.

We have, then, to consider this question: Can double refraction be explained if the statical properties of the æther are independent of the associated matter? Can we suppose that the density within a crystal is a function of the direction of vibration? I answer, yes. The absurdity is apparent only, and disappears on more attentive examination. As I am conscious the position is one that will need all the light that can be thrown upon it, I think it well to give an illustration of a comparatively simple character which occurred to me at an early stage of this inquiry, and which was of great use in showing me in a general way the possibility of the kind of explanation I was in search of.

Let a solid body, such as an ellipsoid, be so supported in space that its centre of inertia is free to move in any direction, but is urged by springs or otherwise towards a certain fixed point with a force symmetrical all round and proportional to the displacement. The arrangement may be supposed to be such that the body always retains its parallelism. Under these circumstances, a vibration may be performed in any direction, and its period is the same in all cases. If the inertia of the body be increased, the only result can be that the motion will become more sluggish and the period longer. Here we have the analogue of singly refracting media. But now suppose that instead of moving in free space the body is immersed in a fluid of sensible density. According to known theorems in hydrodynamics†, the inertia of the fluid adds itself to the inertia of the

* See also Thomson and Tait's 'Natural Philosophy,' vol. i. p. 708.

† Thomson and Tait, p. 262.

body, and that in a manner dependent on the direction of vibration. An extreme case will make this evident. Suppose that the ellipsoid degenerates into a circular disk of inconsiderable thickness. It is clear that if the vibration be performed in a line perpendicular to the disk, the fluid, which cannot readily pass from the one side to the other, will greatly impede the motion—that is, increase its period; for there is no question here of a loss of energy from friction or viscosity.

It is equally evident that if the motion be in the plane of the disk, the fluid has no effect and might as well not be there. We see, then, that, to all intents and purposes, the disk has a density, or rather inertia, of variable magnitude dependent on the line of vibration, and symmetrical round an axis, and are reminded of a uniaxial crystal. Next suppose that we try to make the disk vibrate in a line oblique to itself. It would at once appear that such a vibration cannot be performed without an additional constraint, which we may suppose applied. The system would then perform pendulous vibrations whose period is a function of the position of the line in which the centre of inertia is made to move. Lagrange's general method leads immediately to a solution of the whole problem:—

$$T = \text{kinetic energy} = \frac{1}{2} \{ P\dot{x}^2 + Q(\dot{y}^2 + \dot{z}^2) \},$$

$$V = \text{potential energy} = \frac{\mu}{2} (x^2 + y^2 + z^2);$$

whence the equations of vibratory motion,

$$\left. \begin{aligned} P\ddot{x} + \mu x &= 0, \\ Q\ddot{y} + \mu y &= 0, \\ Q\ddot{z} + \mu z &= 0, \end{aligned} \right\}$$

showing that vibrations along x cannot be performed synchronously with vibrations along y or z . This is on the supposition that the body is free; but if it be constrained to a line ξ making an angle θ with x , we have

$$T = \frac{1}{2} \xi^2 (P \cos^2 \theta + Q \sin^2 \theta),$$

$$V = \frac{\mu}{2} \xi^2,$$

whence

$$(P \cos^2 \theta + Q \sin^2 \theta) \ddot{\xi} + \mu \xi = 0,$$

so that the period τ is

$$2\pi \sqrt{\frac{P \cos^2 \theta + Q \sin^2 \theta}{\mu}}.$$

When $\theta=0$, let $\tau=\tau_1$; when $\theta=90^\circ$, let $\tau=\tau_2$; then

$$\tau^2 = \tau_1^2 \cos^2 \theta + \tau_2^2 \sin^2 \theta.$$

There is, of course, one case which does not bring out the peculiarity for whose sake the illustration is brought forward, I mean when the ellipsoid becomes a sphere. The only effect of the fluid is then to retard the motion, just as if the mass of the sphere itself had been increased.

From the problem generally we may infer that there is nothing absurd in the idea of an inertia varying with the direction of motion, and that the want of symmetry causing double refraction may be attributed with as great probability to the dynamical as to the statical conditions of the question. We know nothing about the real nature of the æther, and, if possible, still less about its relations to ponderable matter; and it is therefore the merest assumption to say that the energy of motion within a crystal is necessarily a symmetrical function of the velocities of displacement. But this has virtually been done in all the theories hitherto given. I would even go further, and ask whether, when we consider the enormous velocity of light and the magnitude of the forces which resist distortion, it is not on the whole more probable that the relatively considerable effect of ponderable matter is due to its action rather on the small quantity (the inertia) than on the great quantity (the rigidity)?

Instead, then, of assuming for the energy of the medium

$$2T = \iiint \rho (\dot{\xi}^2 + \dot{\eta}^2 + \dot{\zeta}^2) dv,$$

let us take the most general quadratic function of $\dot{\xi}, \dot{\eta}, \dot{\zeta}$, containing six constants. Even this form is somewhat restricted; for it may be that the energy cannot be expressed at all as the sum of parts corresponding to the various elements of the æther. Ordinary chromatic dispersion and rotatory polarization, which is a phenomenon of the same nature, show that the mutual influence of the parts is not restricted to a distance which may be regarded as vanishingly small in comparison with the wave-length; and although in Cauchy's theory of dispersion the mutual action is supposed to be of a statical character, yet the fact that there is no dispersion in vacuum, when regarded from the point of view of the present paper, leads rather to the conclusion that the mutual influence is dynamical, by which I mean that it would show itself in the expression of the kinetic rather than of the potential energy. But it will only be following precedents to drop the consideration of dispersion in explaining a theory of double refraction, which may be done consistently by supposing the wave very long.

By a suitable choice of axes the terms involving the products

of the velocities may be got rid of, so that

$$2T = \iiint (\rho_x \dot{\xi}^2 + \rho_y \dot{\eta}^2 + \rho_z \dot{\zeta}^2) dv,$$

where ρ_x, ρ_y, ρ_z are positive quantities representing the densities corresponding to the three coordinate axes. The expression of the potential energy I suppose to be exactly the same as in vacuum; and thus by Lagrange's general method* we find for the equations of motion,

$$\left. \begin{aligned} \rho_x \frac{d^2 \xi}{dt^2} &= a^2 \frac{d\delta}{dx} + b^2 \nabla^2 \xi, \\ \rho_y \frac{d^2 \eta}{dt^2} &= a^2 \frac{d\delta}{dy} + b^2 \nabla^2 \eta, \\ \rho_z \frac{d^2 \zeta}{dt^2} &= a^2 \frac{d\delta}{dz} + b^2 \nabla^2 \zeta. \end{aligned} \right\} \quad . \quad . \quad . \quad (1)$$

On account of the incompressibility of the æther, δ is very small; but it does not follow that the terms containing it are to be omitted, for a^2 is correspondingly great. We may, however, write p for $a^2 \delta$, and p may then be compared to a hydrostatic pressure. The problem of double refraction is solved so soon as the laws are known which regulate the possible directions of vibration and corresponding velocities of propagation for every position of the wave-front.

Let us consider a plane wave whose front is at any time given by

$$lx + my + nz - Vt,$$

so that l, m, n are the direction-cosines of the wave-normal, and V the velocity of propagation. Also let θ denote the actual displacement in the plane of the wave, and $\lambda\mu\nu$ its direction. Thus

$$\xi = \lambda\theta, \quad \eta = \mu\theta, \quad \zeta = \nu\theta;$$

and

$$\left. \begin{aligned} \lambda\rho_x \frac{d^2 \theta}{dt^2} &= \frac{dp}{dx} + b^2 \lambda \nabla^2 \theta, \\ \mu\rho_y \frac{d^2 \theta}{dt^2} &= \frac{dp}{dy} + b^2 \mu \nabla^2 \theta, \\ \nu\rho_z \frac{d^2 \theta}{dt^2} &= \frac{dp}{dz} + b^2 \nu \nabla^2 \theta. \end{aligned} \right\} \quad . \quad . \quad . \quad (2)$$

Now let

$$\left. \begin{aligned} \theta &= \theta_0 e^{i(lx + my + nz - Vt)}, \\ p &= p_0 e^{i(lx + my + nz - Vt)}, \end{aligned} \right\} \quad . \quad . \quad . \quad (3)$$

where θ_0 and p_0 are complex constants. On substitution in (2),

* Thomson and Tait, p. 710. Green, Camb. Trans. 1838.

$$\left. \begin{aligned} \lambda \theta_0 (V^2 \rho_x - b^2) &= -ip_0 l, \\ \mu \theta_0 (V^2 \rho_y - b^2) &= -ip_0 m, \\ \nu \theta_0 (V^2 \rho_z - b^2) &= -ip_0 n; \end{aligned} \right\} \quad . \quad . \quad . \quad (4)$$

and since $\lambda\lambda + m\mu + n\nu = 0$,

$$\frac{l^2}{V^2 \rho_x - b^2} + \frac{m^2}{V^2 \rho_y - b^2} + \frac{n^2}{V^2 \rho_z - b^2} = 0;$$

or if, as in the ordinary notation, a, b, c are the principal velocities of propagation*,

$$\frac{l^2}{\frac{V^2}{a^2} - 1} + \frac{m^2}{\frac{V^2}{b^2} - 1} + \frac{n^2}{\frac{V^2}{c^2} - 1} = 0. \quad . \quad . \quad . \quad (5)$$

The equations determining the directions of vibration are

$$\left. \begin{aligned} \frac{l}{\lambda} \left(\frac{1}{b^2} - \frac{1}{c^2} \right) + \frac{m}{\mu} \left(\frac{1}{c^2} - \frac{1}{a^2} \right) + \frac{n}{\nu} \left(\frac{1}{a^2} - \frac{1}{b^2} \right) &= 0, \\ \lambda\lambda + m\mu + n\nu &= 0. \end{aligned} \right\} \quad . \quad (6)$$

Equations (5) and (6) constitute the analytical solution of the problem. I had originally expected to reproduce in their integrity the beautiful laws of Fresnel; but a slight examination will show that, in order to reconcile (5) and (6) with Fresnel's equations, we must write, for V^2, a^2, b^2, c^2 ,

$$\frac{1}{V^2}, \quad \frac{1}{a^2}, \quad \frac{1}{b^2}, \quad \frac{1}{c^2}$$

respectively. The directions of vibration are parallel to the axes of the section of the ellipsoid

$$\frac{x^2}{a^2} + \frac{y^2}{b^2} + \frac{z^2}{c^2} = 1$$

by the plane of the wave; and the velocities of propagation are *directly* proportional to the lengths of the axes. Accordingly the wave-surface is the envelope of planes drawn parallel to the central sections of the ellipsoid $\frac{x^2}{a^2} + \frac{y^2}{b^2} + \frac{z^2}{c^2} = 1$ at distances directly proportional to the lengths of the axes. Fresnel's surface is the locus of points situated on the normals to the sections of the same ellipsoid and at the same distances. We see, therefore, that the new surface is related to Fresnel's in the following way:—Through any point of the latter draw a plane perpendicular to the line joining it to the centre; the envelope of these planes is the former surface. In the principal planes of a biaxial crystal the new surface agrees with Fresnel's as regards the sec-

* The meaning of b is here changed.

tion which is a circle; but the other is not a true ellipse. Within a uniaxal crystal one ray always follows the ordinary law.

In ordinary media the transversal vibrations can be propagated without any tendency to produce dilatation (positive or negative). But it is not so here. Suppose in our illustration that the centre of the ellipsoid is constrained to move in a certain plane. We should find two directions of possible vibration and two corresponding periods, just as for light in a crystal. The question presents itself, What in the latter case takes the place of the external constraint? *The resistance of the æther to compression*—is the answer. Any part of the æther during the passage of a transverse wave over it tends (except in particular cases) to move normally; but the tendency is shared by all the other parts in the same sheet parallel to the wave-front. The motion, therefore, cannot be actually performed, because it would involve a compression of the medium, which by hypothesis requires an infinite force. The pressure p , however, is not without effect; for it modifies the reflection and refraction when light enters a crystal, and it is probably closely connected with the oblique propagation of a ray in the interior. The actual direction of a ray is to be found from the wave-surface, just as in Fresnel's theory.

I had got about as far as this in my original work when, on reference to Professor Stokes's report, I was greatly surprised to find allusions to a theory of double refraction mathematically, if not physically, identical with that here advanced. After insisting on the importance of precise measurements, he says:—"To make my meaning clearer, I will refer to Fresnel's construction, in which the laws of polarization and wave-velocity are determined by the sections, by a diametral plane parallel to the wave-front, of the ellipsoid

$$a^2x^2 + b^2y^2 + c^2z^2 = 1, \quad . \quad . \quad (11)$$

where a, b, c denote the principal wave-velocities. The principal semi-axes of the section determine by their direction the normals to the two planes of polarization, and by their magnitude the reciprocals of the corresponding wave-velocities. Now a certain other physical theory which might be proposed leads to a construction differing from Fresnel's only in this, that the planes of polarization and wave-velocities are determined by the section, by a diametral plane parallel to the wave-front, of the ellipsoid

$$\frac{x^2}{a^2} + \frac{y^2}{b^2} + \frac{z^2}{c^2} = 1, \quad . \quad . \quad (12)$$

the principal semi-axes of the section determining by their direction the normals to the two planes of polarization, and by their magnitudes the corresponding wave-velocities. The law that the planes of polarization of the two waves propagated in a given di-

rection bisect respectively the two supplemental dihedral angles made by planes passing through the wave-normal and the two optic axes, remains the same as before; but the positions of the optic axes themselves, as determined by the principal indices of refraction, are somewhat different; the difference, however, is but small if the differences between a^2 , b^2 , c^2 are a good deal smaller than the quantities themselves. Each principal section of the wave-surface, instead of being a circle and an ellipse, is a circle and an oval, to which an ellipse is a near approximation. The difference between the inclinations of the optic axes and between the amounts of extraordinary refraction in the principal planes, on the two theories, though small, are quite sensible in observation, but only on condition that the observations are made with great precision. We see from this example of what great advantage for the advancement of theory observations of this character may be."

And again:—

"The curious and unexpected phenomenon of conical refraction has justly been regarded as one of the most striking proofs of the general correctness of the conclusions resulting from the theory of Fresnel. But I wish to point out that the phenomenon is not competent to decide between several theories leading to Fresnel's construction as a near approximation. . . . We see, therefore, that the limitation of the number of tangent planes to the wave-surface which can be drawn in a given direction on one side of the centre to two, or at the most three, is intimately bound up with the number of dimensions of space; so that the existence of the phenomenon of internal conical refraction is no proof of the truth of the particular form of wave-surface assigned by Fresnel rather than that to which some other theory would conduct. Were the law of wave-velocity expressed, for example, by the construction already mentioned having reference to the ellipsoid (12), the wave-surface (in this case a surface of the 16th degree) would still have plane curves of contact with the tangent-plane, which in this case also, as in the wave-surface of Fresnel, are, as I find, circles, though that they should be circles could not have been foreseen.

"The existence of external conical refraction depends upon the existence of a conical point in the wave-surface, by which the interior sheet passes to the exterior. The existence of a conical point is not, like that of a plane curve of contact, a necessary property of a wave-surface. Still it will readily be conceived that if Fresnel's wave-surface be, as it undoubtedly is, at least a near approximation to the true wave-surface, and if the latter have, moreover, plane curves of contact with the tangent plane, the mode by which the exterior sheet passes within one of these plane curves

into the interior will be very approximately by a conical point; so that in the impossibility of operating experimentally on mere rays the phenomena will not be sensibly different from what they would have been had the transition been made rigorously by a conical point."

Between the theory here advanced and that of Fresnel observation ought to decide; but it does not appear that any experiments hitherto made are competent to do so. As Professor Stokes points out, all the measurements which are to be combined in one calculation should refer to the same specimen of the crystal; otherwise an element of uncertainty is introduced sufficient to render the application of the test ambiguous. Should the verdict go against the view of the present paper, it is hard to see how any consistent theory is possible, which shall embrace at once the laws of scattering, regular reflection, and double refraction.

LXVI. *Boiling-points of Organic Bodies.*

By F. BURDEN, Esq.*

SINCE it was observed that the boiling-points of each homologous series of organic bodies was higher for every addition to the substance of CH_2 , several continental chemists (but especially M. Kopp) have tried to discover the law which connects the boiling-point with the composition. No proportionate relation has yet been found to connect different series; and yet I believe I can show that the greater number of those organic bodies which contain only carbon, hydrogen, and oxygen obey a law of great generality and simplicity, and that there are reasons for believing that further experiments will reduce the number of exceptions.

It has been calculated by Joule and Clausius, on the hypothesis of a motion of translation in gaseous molecules, that the velocity of the atoms of hydrogen necessary to balance a pressure of 15 lbs. per square inch at the temperature of 0°C . is 6050 feet per second. And it has been assumed by them, on mechanical grounds, and verified experimentally by Graham, that the product of the specific gravity of any gas upon the square of the velocity is a constant, under the same circumstances of temperature and pressure. As the specific gravity of hydrogen is taken as unity, this constant must be 36,602,500. The velocity of the molecules of any vapour at 0°C . can therefore be obtained by dividing this constant by the specific gravity and extracting the square root. In this way was calculated the fourth column of the accompanying Tables.

It has been further deduced from known laws that this velocity

* Communicated by the Author.

is increased by $\frac{1}{273}$ of the velocity at 0° C. for each degree the vapour is raised above this point. Therefore the velocity of the molecules of any vapour at its boiling-point may be calculated by dividing the number in the fourth column by 273, multiplying the quotient by the number which indicates the boiling-point, and adding the result to the number in the fourth column. By this means were obtained the numbers in the sixth column of the Tables.

A comparison of the Tables for paraffins, olefines, aromatic and other hydrocarbons, and the ethers (simple, mixed, and compound) shows such a uniformity as clearly indicates a law. The very small difference is less than might have been expected from the difficulties in the way of obtaining the exact boiling-points.

The law which I would deduce from these results may be expressed thus:—When any hydrocarbon or oxygen ether in a state of vapour, under a barometric pressure of 760 millims., is gradually cooled until the velocity of its molecules is reduced to 1140 feet per second, then, and not till then, has the force of cohesion power to condense it into a liquid.

It is an easy algebraical deduction from this that the boiling-point of any of these bodies may be found by multiplying the square root of the specific gravity by a constant which I have calculated at 51.5, and deducting 273° ; and whatever may be thought of the hypothesis by which I have endeavoured to connect the facts with molecular theories, the fact remains that by this formula a Table of boiling-points for these bodies may be constructed which will not differ from the results of experiment more than these differ from one another. As a proof of this, I have added a number of supplemental Tables, in which the calculated boiling-points are compared with those observed by the experimenter whose name follows.

The remaining Tables show a considerable difference from these results. The alcohols and acids especially give a much higher velocity at their boiling-points. There are grounds for believing, however, that these series are not so exceptional as they at first appear. M. Bineau, in a paper in the *Comptes Rendus*, states that the acids belonging to the series $\text{H C}^n\text{H}^{(2n-1)}\text{O}^2$ exhibit a remarkable variation in the volume of vapour which they furnish at different temperatures. Thus formic acid at its boiling-point has a specific gravity of 46 in place of 23, and regains its proper bulk only at a temperature of 213° . Acetic acid at its boiling-point has a specific gravity of 57 instead of 30. If we make the alteration required by these facts in the Table for acids, formic acid will show at its boiling-point a velocity of 1160 feet, and acetic acid a velocity of 1134 feet per second.

The experiments of M. H. Herwig show that ethyl alcohol

also possesses an exceptional density; but as the published account of these experiments gives the results only at low pressures, I am unable to make the same correction as in the case of the above-mentioned acids.

TABLE A.—Paraffins.

Name.	Formula.	Specific gravity.	Velocity at 0°.	Boiling-point.	Velocity at boiling-point.
Quartane	$C^4 H^{10}$	29	1124	above 0	1136
Quintane	$C^5 H^{12}$	36	1008	30	1119
Sextane	$C^6 H^{14}$	43	922	68	1152
Septane	$C^7 H^{16}$	50	855	92	1143
Octane	$C^8 H^{18}$	57	801	116	1142
Nonane	$C^9 H^{20}$	64	756	138	1136
Decane	$C^{10} H^{22}$	71	718	162	1142
Undecane	$C^{11} H^{24}$	78	692	182	1149
Duodecane	$C^{12} H^{26}$	85	657	200	1137
Tridecane	$C^{13} H^{28}$	92	633	218	1143
Quatuordecane	$C^{14} H^{30}$	99	602	240	1132
Quindecane	$C^{15} H^{32}$	106	584	258	1130

Mean velocity 1138 feet per second.

TABLE B.—Olefines.

Quartene	$C^4 H^8$	28	1143	3	1155
Quintene	$C^5 H^{10}$	35	1022	35	1153
Sextene	$C^6 H^{12}$	42	932	68	1164
Septene	$C^7 H^{14}$	49	864	95	1163
Octene	$C^8 H^{16}$	56	807	115	1152
Nonene	$C^9 H^{18}$	63	762	140	1154
Decene	$C^{10} H^{20}$	70	723	160	1147
Sexdecene	$C^{16} H^{32}$	112	573	275	1150

Mean velocity 1155 feet per second.

TABLE C.—Other Hydrocarbons.

Quartine	$C^4 H^6$	27	1160	15	1224
Quintine	$C^5 H^8$	34	1037	45	1208
Sextine	$C^6 H^{10}$	41	945	58	1150
Quintone	$C^5 H^6$	33	1053	50	1144
Terebenthine	$C^{10} H^{16}$	68	733	161	1165
Metaterebenthine	$C^{20} H^{32}$	136	520	360	1204

Mean velocity 1182 feet per second.

TABLE D.—Aromatic Hydrocarbons.

Benzine	$C^6 H^6$	39	968	83	1259
Toluene	$C^7 H^8$	46	892	111	1258
Xylene	$C^8 H^{10}$	53	830	139	1253
Cumene	$C^9 H^{12}$	60	781	152	1213
Cymene	$C^{10} H^{14}$	67	739	175	1211
Amylbenzine	$C^{11} H^{16}$	74	703	195	1265

Mean velocity 1243 feet per second.

TABLE E.—Simple Ethers.

Name.	Formula.	Specific gravity.	Velocity at 0°.	Boiling-point.	Velocity at boiling-point.
Methyl ether	$C^2 H^6 O$	23	1264	—21°	1168
Methylethyl ether	$C^3 H^8 O$	30	1099	11	1143
Ethyl ether	$C^4 H^{10} O$	37	990	34	1112
Methylamyl ether	$C^5 H^{12} O$	44	909	72	1149
Ethyltetryl ether	$C^6 H^{14} O$	51	845	80	1093
Ethylamyl ether	$C^7 H^{16} O$	58	794	112	1119
Tetryl ether	$C^8 H^{18} O$	65		Doubtful	
Amyl ether.....	$C^{10} H^{22} O$	79	680	183	1135

Mean velocity 1131 feet per second.

TABLE F.—Methylic Ethers.

Methyl formiate	$C^2 H^4 O^2$	30	1099	33	1232
„ acetate	$C^3 H^6 O^2$	37	996	60	1215
„ propionate.....	$C^4 H^8 O^2$	44	909	Unknown.	
„ butyrate.....	$C^5 H^{10} O^2$	51	845	102	1161
„ valerate	$C^6 H^{12} O^2$	58	794	116	1131

Mean velocity 1185 feet per second.

TABLE G.—Ethylic Ethers.

Ethyl formiate	$C^3 H^6 O$	37	996	53	1189
„ acetate	$C^4 H^8 O^2$	44	909	74	1131
„ propionate	$C^5 H^{10} O^2$	51	845	101	1158
„ butyrate	$C^6 H^{12} O^2$	58	794	119	1119
„ valerate.....	$C^7 H^{14} O^2$	65	750	132	1113
„ caproate	$C^8 H^{16} O^2$	72	713	162	1129
„ pelargonate	$C^{11} H^{22} O^2$	93	627	224	1142

Mean velocity 1140 feet per second.

TABLE H.—Other Ethers.

Propyl acetate	$C^5 H^{10} O^2$	51	845	90	1124
Propyl butyrate.....	$C^7 H^{14} O^2$	65	750	130	1108
Butyl formiate	$C^5 H^{10} O^2$	51	845	100	1155
Butyl acetate.....	$C^6 H^{12} O^2$	58	794	114	1127
Octyl acetate	$C^{10} H^{20} O^2$	86	652	193	1112
Ethyl laurate.....	$C^{14} H^{28} O^2$	114	566	269	1125

Mean velocity 1125 feet per second.

TABLE I.—Compound Ethers.

Amyl formiate	$C^6 H^{12} O^2$	60	781	114	1117
„ acetate	$C^7 H^{14} O^2$	67	738	138	1111
„ propionate.....	$C^9 H^{18} O^2$	74	703	155	1102
„ valerate	$C^{10} H^{20} O^2$	88	645	190	1188
„ caproate	$C^{11} H^{22} O^2$	95	620	211	1100

Mean velocity 1123 feet per second.

TABLE K.—Anhydrides.

Name.	Formula.	Specific gravity.	Velocity at 0°.	Boiling-point.	Velocity at boiling-point.
Acetic anhydride	$C^4 H^6 O^3$	51	845	137	1270
Propionic anhydride	$C^6 H^{10} O^3$	65	750	169	1218
Butyric anhydride	$C^8 H^{14} O^3$	79	680	190	1155
Valeric anhydride	$C^{10} H^{18} O^3$	93	627	215	1121
Caprylic anhydride	$C^{16} H^{30} O^3$	135	520	290	1071

Mean velocity 1167 feet per second.

TABLE L.—Aldehydes.

Acetic aldehyde	$C^2 H^4 O$	22	1290	21	1389
Propionic aldehyde	$C^3 H^6 O$	29	1124	55	1351
Butyric aldehyde	$C^4 H^8 O$	36	1008	68	1260
Valeric aldehyde	$C^5 H^{10} O$	43	922	90	1226
Enanthylic aldehyde	$C^7 H^{14} O$	57	801	155	1255
Caprylic aldehyde	$C^8 H^{16} O$	64	756	171	1226
Rutic aldehyde	$C^{10} H^{20} O$	78	692	230	1276

TABLE M.—Alcohols.

Methyl alcohol	$C H^4 O$	16	1512	61	1848
Ethyl alcohol	$C^2 H^6 O$	23	1264	78	1623
Propyl alcohol	$C^3 H^8 O$	30	1099	96	1488
Butyl alcohol	$C^4 H^{10} O$	37	996	109	1392
Amyl alcohol	$C^5 H^{12} O$	44	909	130	1300
Hexyl alcohol	$C^6 H^{14} O$	51	845	148	1304
Heptyl alcohol	$C^7 H^{16} O$	58	794	178	1307
Octyl alcohol	$C^8 H^{18} O$	65	750	182	1251

TABLE N.—Acids.

Formic acid	$C H^2 O^2$	23	1264	100	1726
Acetic acid	$C^2 H^4 O^2$	30	1099	116	1567
Propionic acid	$C^3 H^6 O^2$	37	996	142	1516
Butyric acid	$C^4 H^8 O^2$	44	909	156	1377
Valeric acid	$C^5 H^{10} O^2$	51	845	175	1387
Caproic acid	$C^6 H^{12} O^2$	58	794	198	1370
Enanthylic acid	$C^7 H^{14} O^2$	65	750	218	1350
Caprylic acid	$C^8 H^{16} O^2$	72	713	236	1326
Pelargonic acid	$C^9 H^{18} O^2$	79	680	260	1330

Mean velocities.

Paraffins	1138	feet per second.
Olefines	1155	"
Other hydrocarbons	1182	"
Aromatic hydrocarbons...	1243	"
Simple ethers	1131	"
Methylic ethers	1185	"
Ethylic ethers	1140	"
Other ethers	1125	"
Compound ethers	1123	"
Anhydrides	1167	"

Supplemental Tables, containing the Calculated with the Observed Boiling-points.

TABLE P.—Paraffins.

Name.	Formula.	Specific gravity.	Calculated boiling-point.	Observed boiling-point.	Observer.
Quartane.....	$C^4 H^{10}$	29	4°	A little above 0°	Pelouze and Cahours.
Quintane.....	$C^5 H^{12}$	36	36	30°	"
Sextane	$C^6 H^{14}$	43	65	68	"
Septane	$C^7 H^{16}$	50	91	92	"
Octane	$C^8 H^{18}$	57	116	116	"
Nonane	$C^9 H^{20}$	64	141	138	"
Decane	$C^{10} H^{22}$	71	161	162	"
Undecane	$C^{11} H^{24}$	78	182	182	"
Duodecane	$C^{12} H^{26}$	85	202	200	"
Tridecane	$C^{13} H^{28}$	92	221	218	"
Quatuordecane ...	$C^{14} H^{30}$	99	239	240	"
Quindecane	$C^{15} H^{32}$	106	257	258	"

TABLE Q.—Olefines.

Quartene	$C^4 H^8$	28	0	3	"
Quintene.....	$C^5 H^{10}$	35	32	35	"
Sextene	$C^6 H^{12}$	42	61	68	"
Septene	$C^7 H^{14}$	49	88	95	"
Octene	$C^8 H^{16}$	56	112	115	"
Nonene	$C^9 H^{18}$	63	136	140	"
Decene	$C^{10} H^{20}$	70	158	160	"
Sexdecene	$C^{16} H^{32}$	112	273	275	"

TABLE R.—Simple Ethers.

Methyl ether	$C^2 H^6 O$	23	-26	-21	Berthelot.
Methylethyl ether.	$C^3 H^8 O$	30	9	11	Williamson.
Ethyl ether.....	$C^4 H^{10} O$	37	40	36	Gay-Lussac.
Methylamyl ether.	$C^5 H^{12} O$	44	68	72	Williamson.
Ethyltetryl ether..	$C^6 H^{14} O$	51	95	80	Wurtz.
Ethylamyl ether...	$C^7 H^{16} O$	58	118	112	Williamson.
Tetryl ether	$C^8 H^{18} O$	65	142	Doubtful.	
Amyl ether.....	$C^{10} H^{22} O$	79	184	182	Rieckher.

TABLE S.—Methylic Ethers.

Methyl formiate ...	$C^2 H^4 O^2$	30	9	33	Andrews,
" acetate ...	$C^3 H^6 O^2$	37	40	55	Andrews.
" propionate	$C^4 H^8 O^2$	44	68	Unknown.	
" butyrate ...	$C^5 H^{10} O^2$	51	95	95	Kopp.
" valerate ...	$C^6 H^{12} O^2$	58	118	116	Kopp.

TABLE T.—Ethylic Ethers.

Name.	Formula.	Specific gravity.	Calculated boiling-point.	Observed boiling-point.	Observer.
Ethyl formiate ...	$C^3 H^6 O^2$	37	40	53	{ Pierre and Delffs
„ acetate	$C^4 H^8 O^2$	44	68	74	Kopp.
„ propionate ...	$C^5 H^{10} O^2$	41	95	96	Kopp.
„ butyrate ...	$C^6 H^{12} O^2$	58	118	119	Pierre.
„ valerate ...	$C^7 H^{14} O^2$	65	142	134	Berthelot.
„ caproate ...	$C^8 H^{16} O^2$	72	164	162	Fehling.
„ pelargonate.	$C^{11} H^{22} O^2$	93	224	224	Delffs.

TABLE V.—Other Ethers.

Propyl acetate ...	$C^5 H^{10} O^2$	51	95	90	Berthelot.
Propyl butyrate...	$C^7 H^{14} O^2$	65	142	130	Berthelot.
Butyl formiate ...	$C^5 H^{10} O^2$	51	95	100	Wurtz.
Butyl acetate	$C^6 H^{12} O^2$	58	119	114	Wurtz.
Octyl acetate	$C^{10} H^{20} O^2$	86	204	193	Bouis.
Ethyl laurate.....	$C^{14} H^{28} O^2$	114	277	269	Delffs.

TABLE W.—Compound Ethers.

Amyl formiate ...	$C^6 H^{12} O^2$	60	126	116	Kopp.
„ acetate	$C^7 H^{14} O^2$	67	149	137	Kopp.
„ propionate ...	$C^8 H^{16} O^2$	74	170	about 155	Wrightson.
„ valerate ...	$C^{10} H^{20} O^2$	88	210	189	Kopp.
„ caproate ...	$C^{11} H^{22} O^2$	95	229	211	Brazier.

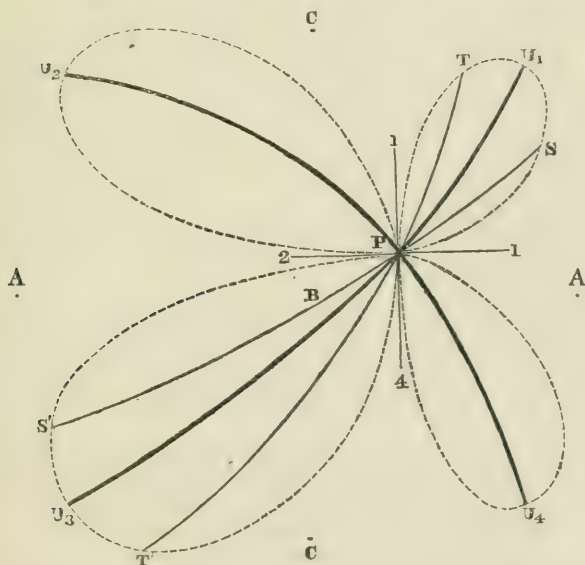
LXVII. *Note on the Geodesic Lines on an Ellipsoid.*

By Professor CAYLEY, F.R.S.*

THE general configuration of the geodesic lines on an ellipsoid is established by means of the known theorem) an immediate consequence of Jacobi's fundamental formulæ, but which was first given by Mr. Michael Roberts, *Comptes Rendus*, vol. xxi. p. 1470, Dec. 1845) that every geodesic line touches a curve of curvature; that is, attending to the two opposite ovals which constitute the curve of curvature, the geodesic line is in general an infinite curve undulating between these opposite ovals, and so touching each of them an infinite number of times (but possibly in particular cases it is a reentrant curve touching each oval a finite number of times). The geodesic lines thus divide themselves into two kinds, accordingly as they touch a curve of curvature of the one or the other kind; and there is besides a

* Communicated by the Author.

third limiting kind, the lines which pass through an umbilicus: any such geodesic line passes through the opposite umbilicus, and is in general an infinite curve passing an infinite number of times alternately through the two umbilici; but possibly it is in particular cases a reentrant curve passing a finite number of times through the two umbilici. I annex a figure giving a general idea of the configuration of the geodesic lines drawn in different directions from a given point P on the surface of the ellipsoid: this is drawn (as it were) on the plane of the greatest and least axes; but it is not a perspective or geometrical representation of any kind, but a mere diagram for the purpose in question. We have A, A, B, C, C the extremities of the axes; U_1, U_2, U_3, U_4 the umbilici; P the point on the surface; $1 P 2$ and $1 P 4$ the curves of curvature through P , viz. these are ovals containing the umbilici U_1, U_2 and U_1, U_4 respectively. Then



$U_1 P U_3$ and $U_2 P U_4$ are the limiting geodesics passing through the umbilici; the line $T P T'$ represents a geodesic line of the one kind, viz. this at T touches an oval (curve of curvature) $U_1 U_4$, and at T' the conjugate oval $U_2 U_3$. Similarly $S P S'$ is a geodesic line of the other kind, viz. this at S touches an oval (curve of curvature) $U_1 U_2$, and at S' the conjugate oval $U_3 U_4$; the dotted figure-of-eight curves are the loci of the points of contact T, T', S, S' .

LXVIII. *Notices respecting New Books.*

The Meteoric Theory of Saturn's Rings considered with reference to the Solar Motion in Space, also a paper on the Meteoric Theory of the Sun. By Lieut. AUGUSTUS MORSE DAVIES, B.A., R.A., &c. Longmans, 1871.

THIS work, illustrated by nineteen plates, is, as its title imports, nearly pure theory. Based upon the assumption, which the author considers he is justified "in making of the solar motion being in reality a curve," he builds up his theory of the rings of Saturn having resulted from the planet arresting groups of meteors which it encountered as they descended towards the sun.

The work is divided into sections, the first containing brief notices of the successive theories which have been broached from the time of Maupertius to that of Proctor, three hypotheses having been submitted, viz. the solid, the fluid, and the satellitic. In connexion with the last, Lieut. Davies has the following remark:—"I have thus far attempted to sketch very briefly the satellite theory of Saturn's rings; and here the question naturally arises, Whence has this vast cloud of minute satellites been drawn, and how has it been subjected to Saturn's dominion? Has Saturn always been attended by these magnificent rings, or has some fortuitous encounter with space-wandering meteors enabled it, at some remote period, to enrich the splendour of its eight-mooned orb?" The remaining portion of the section is occupied with considerations tending to show that, of the four large and exterior planets, Saturn is more favourably situated for arresting and retaining groups of meteors than either Jupiter, Uranus, or Neptune.

In Section II. Lieut. Davies proceeds to determine the absolute path of Saturn in space upon the assumption of the solar curve, and chooses for the epoch that in which the major axis and the line of the solar motion lie in the same vertical plane, the result of his calculations being that Saturn's motion in space is always in the same direction as the sun's.

Section III. contains the geometrical interpretation of the result obtained in the preceding section, from which it appears that the four exterior planets trace out progressive spirals in space.

In Section IV. the author arrives at the conclusion that if an encounter with meteors has occurred, Saturn, of all the planets, would be most likely to accumulate them as a ring system.

Section VI. contains the investigation of the circumstances of the attachment of the meteors to the Saturnian system, from which Lieut. Davies finds that, at a point 59,000 miles from the centre of Saturn, the velocity of the meteors is about 61,592·4 miles per hour. "Hence," he says, "if we subtract from this their velocity in common with Saturn in a direction at right angles to their motion at this point, we shall have 33,378·66 miles per hour for the velocity of the meteors at that point in their orbit round Saturn, their calculated velocity being 33,714·3 miles per hour. This is, I think, conclusive as to the meteoric origin of the rings."

Having arrived at the conclusion that Saturn's rings originated from an encounter of the planet with a group of meteors, the author seeks for information relative to the period of attachment. He conceives that the Saturnian meteors, and those now composing the zodiacal light, entered the solar system together, at a period considerably anterior to that of the sun's first luminosity, but not much anterior to the earth's early fluid state. "Geologists," he says, "have given the duration of the period throughout which the earth has borne living species, variously from one to nine millions of years. We may therefore conclude that the formation of Saturn's meteoric rings has occurred not much more recently than two millions of years ago, and probably at a date not very much more remote than nine millions of years. The earth's original fluid state was probably owing to a meteoric storm due to the entry of the meteors into the solar system."

To the meteoric theory of Saturn's rings is appended a paper on the meteoric theory of the sun, in which the author considers that the source of energy from which solar heat is derived is undoubtedly meteoric, and that the solar spots result from the absence of meteoric streams falling into the sun, which make themselves manifest as "willow leaves,"—also that the periodicity of sun-spots is accounted for by a periodicity of the meteoric flights, fewer meteors falling on the sun during the maximum period of spots.

We have in the preceding paragraphs endeavoured to give a faithful analysis of the work before us. With regard to the assumption on which the author's conclusions are based, we leave it with his readers to exercise their own judgment. Mr. Proctor has lately broached a theory of solar eruption, in which he considers that the solar coronal matter consists of meteors *ejected* from the sun, and rushing through the photosphere with a velocity of 200 miles a second, the initial velocity (generated at a point far below the photosphere) being not less than 300 miles a second. Lieut. Davies speaks of the probable velocity of a meteor falling into the sun from space just prior to its final dissipation on the surface as ranging from 246 to 390 miles a second. The two theories are now before the public, and will doubtless furnish material for further and closer investigation.

LXIX. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 398.]

January 19, 1871.—General Sir Edward Sabine, K.C.B., President, in the Chair.

THE following communications were read:—

"Modification of Wheatstone's Bridge to find the Resistance of a Galvanometer-Coil from a single deflection of its own needle." By Prof. Sir William Thomson, F.R.S.

In any useful arrangement in which a galvanometer or electrometer and a galvanic element or battery are connected, through whatever

train or network of conductors, let the galvanometer and battery be interchanged, another arrangement is obtained which will probably be useful for a very different, although reciprocally related object. Hence, as soon as I learned from Mr. Mance his admirable method of measuring the internal resistance of a galvanic element (that described in the first of his two preceding papers *), it occurred to me that the reciprocal arrangement would afford a means of finding the resistance of a galvanometer-coil, from a single deflection of its own needle, by a galvanic element of unknown resistance. The resulting method proves to be of such extreme simplicity that it would be incredible that it had not occurred to any one before, were it not that I fail to find any trace of it published in books or papers, and that personal inquiries of the best-informed electricians of this country have shown that, in this country at least, it is a novelty. It consists simply in making the galvanometer-coil one of the four conductors of a Wheatstone's bridge, and adjusting, as usual, to get the zero of current when the bridge-contact is made—with only this difference, that the test of the zero is not by a galvanometer in the bridge showing no deflection, but by the galvanometer itself, the resistance of whose coil is to be measured, showing an unchanged deflection. Neither diagram nor further explanation is necessary to make this understood to any one who knows Wheatstone's bridge.

“On a Constant Form of Daniell's Battery.” By Prof. Sir William Thomson, F.R.S.

Graham's discovery of the extreme slowness with which one liquid diffuses into another, and Fick's mathematical theory of diffusion, cannot fail to suggest that diffusion alone, without intervention of a porous cell or membrane, might be advantageously used for keeping the two liquids of a Daniell's battery separate. Hitherto, however, no galvanic element without some form of porous cell, membrane, or other porous solid for separator, has been found satisfactory in practice.

The first idea of dispensing with a porous cell, and keeping the two liquids separate by gravity, is due to Mr. C. F. Varley, who proposed to put the copper plate in the bottom of a jar, resting on it a saturated solution of sulphate of copper, resting on this a less dense solution of sulphate of zinc, and, immersed in the sulphate of zinc, the metal zinc plate fixed near the top of the jar. But he tells me that batteries on this plan, called “gravity-batteries,” were carefully tried in the late Electric and International Telegraph Company's establishments, and found wanting in economy. The waste of zinc and of sulphate of copper was found to be more in them than in the ordinary porous-cell batteries. Daniell's batteries without porous cells have also been tried in France, and found unsatisfactory on account of the too free access of sulphate of copper to the zinc, which they permit. Still, Graham's and Fick's measurements leave no room to doubt that the access of sulphate of copper to the zinc would be much less rapid, if by true diffusion alone, than it cannot but be in any form of porous-cell battery with vertical plates of copper and zinc opposed to one another as are the ordinary telegraphic Daniell's batteries which Mr. Varley finds

* Phil. Mag. April 1871, pp. 314, 318.

superior to his own "gravity-battery." The comparative failure of the latter, therefore, must have arisen from mixing by currents of the liquids. All that seems necessary, therefore, to make the gravity-battery much superior instead of somewhat inferior to the porous-cell battery, is to secure that the lower part of the liquid shall always remain denser than the upper part. In seeking how to realize this condition, it first occurred to me to take advantage of the fact that saturated solution of sulphate of zinc is much denser than saturated solution of sulphate of copper. It seems* that, at 15° temperature, saturated aqueous solution of sulphate of copper is of 1.186 sp. gr., and contains in every 100 parts of water 33.1 parts of the crystalline salt, and that at 15° the saturated solution of sulphate of zinc is of sp. gr. 1.44 and contains in every 100 parts of water 140.5 parts of sulphate of zinc—both results being from Michel and Krafft's experiments†. Hence I made an element with the zinc below, next it saturated solution of sulphate of zinc, gradually diminishing to half strength through a few centimetres upwards, saturated sulphate of copper resting on this, and the copper plate fixed above in the sulphate-of-copper solution. In the beginning, and for some time after, it is clear that the sulphate of copper can have no access to the zinc otherwise than by true diffusion. I have found this anticipation thoroughly realized in trials continued for several weeks; but the ultimate fate of such a battery is that the sulphate of zinc must penetrate through the whole liquid; and then it will be impossible to keep sulphate of copper separate in the upper part, because saturated solution of sulphate of zinc certainly becomes denser on the introduction of sulphate of copper to it. To escape this chaotic termination I have introduced a siphon of glass with a piece of cotton-wick along its length inside it, so placed as to draw off liquor very gradually from a level somewhat nearer the copper than the zinc—and a glass funnel, also provided with a core of cotton wick, by which water semisaturated with sulphate of zinc may be continually introduced at a somewhat lower level. A galvanic element thus arranged will undoubtedly continue remarkably constant for many months; but it has one defect, which prevents me from expecting permanence for years. The zinc being below, must sooner or later, according to the less or greater vertical dimensions of the cell, become covered with precipitated copper from the sulphate of copper, which finds its way (however slowly) to the zinc. On the other hand, if the zinc be above, the greater part of the deposited copper falls off incoherently from the zinc through the liquid to the copper below, where it does no mischief, provided always that the zinc be not amalgamated—a most important condition for permanent batteries, pointed out to me many years ago by Mr. Varley. Placing the zinc above has also the great practical advantage that, even when after a very long time it becomes so much coated with metallic copper as to seriously injure the electrical effect, it may be removed, cleaned, and replaced without otherwise disturbing

* Storer's Dictionary of Solubilities of Chemical Substances. Cambridge, Massachusetts: Sever and Francis, 1864.

† Ann. Ch. et Phys. (3) vol. xli. pp. 478, 482: 1854.

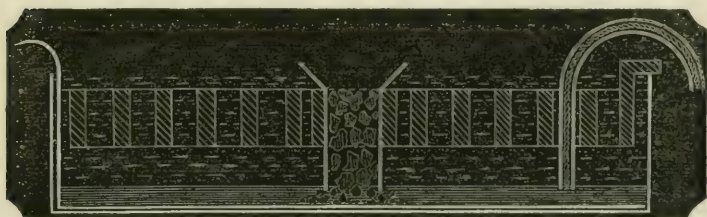
the cell; whereas if the zinc be below, it cannot be cleaned without emptying the cell and mixing the solutions, which will entail a renewal of fresh separate solutions in setting up the cell again. I have therefore planned the following form of element, which cannot but last until the zinc is eaten away so much as to fall to pieces, and which must, I think, as long as it lasts, have a very satisfactory degree of constancy.

The cell is of glass, in order that the condition of the solutions and metals which it contains may be easily seen at any time. It is simply a cylindrical or rectangular jar with a flat bottom. It need not be more than 10 centimetres deep; but it may be much deeper, with advantage in respect to permanence and ease of management, when very small internal resistance is not desired. A disk of thin sheet copper is laid at its bottom. A properly shaped mass of zinc is supported in the upper part of the jar. A glass tube (which for brevity will be called the charging-tube) of a centimetre or more internal diameter, ending in a wide saucer or funnel above, passes through the centre of the zinc, and is supported so as to rest with its lower open end about a centimetre above the copper. A glass siphon with cotton-wick core is placed so as to draw liquid gradually from a level about a centimetre and a half above the copper. The jar is then filled with semisaturated sulphate-of-zinc solution. A copper wire or stout ribbon of copper coated with india-rubber or gutta percha passes vertically down through the liquid to the copper plate below, to which it is riveted or soldered to secure metallic communication. Another suitable electrode is kept in metallic communication with the zinc above. To put the cell in action, fragments of sulphate of copper, small enough to fall down through the charging-tube, are placed in the funnel above. In the course of a very short time the whole liquid below the lower end of the charging-tube becomes saturated with sulphate of copper, and the cell is ready for use. It may be kept always ready by occasionally (once a week for instance) pouring in enough of fresh water, or of water quarter saturated with sulphate of zinc at the top of the cell, to replace the liquid drawn off by the siphon from near the bottom. A cover may be advantageously added above, to prevent evaporation. When the cell is much used, so that zinc enough is dissolved, the liquid added above may be pure water; or if large internal resistance is not objected to, the liquid added may be pure water, whether the cell has been much used or not; but after any interval, during which the battery has not been much in use, the liquid added ought to be quarter-saturated, or even stronger, solution of sulphate of zinc when it is desired to keep down the internal resistance. It is probable that one or more specific-gravity beads kept constantly floating between top and bottom of the heterogeneous fluid will be found a useful adjunct, to guide in judging whether to fill up with pure water or with sulphate-of-zinc solution. They may be kept in a place convenient for observation by caging them in a vertical glass tube perforated sufficiently to secure equal density in the horizontal layers of liquid, to be tested by the floaters.

An extemporized cell on this plan was exhibited to the Royal Society; and its resistance (measured as an illustration of Mance's method, described in the first of his two previous communications) was found to be $\cdot 29$ of an Ohm (that is to say, 290,000,000 centimetres per second). The copper and zinc plates of this cell, being circular, were about 30 centimetres in diameter, and the distance between them was about 7.5 centimetres. A Grove's cell, of such dimensions that forty in series would give an excellent electric light, was also measured for resistance, and found to be $\cdot 19$ of an Ohm. Its intensity was found to be 1.8 times that of the new cell, which is the usual ratio of Grove's to Daniell's; hence seventy-two of the new cells would have the intensity of forty of Grove's. But the resistance of the seventy-two in series would be 209 Ohms, as against 76 Ohms of the forty Grove's; hence, to get as powerful an electric light, threefold surface, or else diminished resistance by diminished distance of the plates, would be required. How much the resistance may be diminished by diminishing the distance rather than increasing the surface, it is impossible to deduce from the experiments hitherto made.

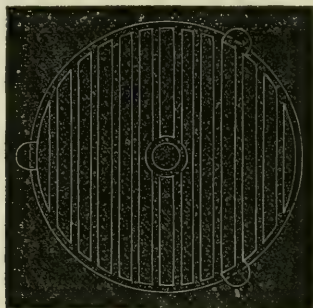
Two or three cells, such as the one shown to the Royal Society, will be amply sufficient to drive a large ordinary turret-clock without a weight; and the expense of maintaining them will be very small in comparison with that of winding the clock. The prime cost of the heavy wheelwork will be avoided by the introduction of a comparatively inexpensive electromagnetic engine. For electric bells, and all telegraphic testing and signaling on shore, the new form of battery will probably be found easier of management, less expensive, and more trustworthy than any of the forms of battery hitherto used. For use at sea, it is probable that the sawdust Daniell's, first introduced on board the 'Agamemnon' in 1858, and ever since that time very much used both at sea and on shore, will still be found the most convenient form; but the new form is certainly better for all ordinary shore uses.

The accompanying drawing represents a design suitable for the electric light, or other purposes, for which an interior resistance not exceeding $\frac{2}{10}$ of an Ohm is desired. The zinc is in the form of a



grating, to prevent the lodgment of bubbles of hydrogen gas, which I find constantly, but very slowly, gathering upon the zincs of the cells I have tried, although the solutions used have no free acid,

unless such as may come from the ordinary commercial sulphate-of-copper and commercial sulphate-of-zinc crystals which were used.



POSTSCRIPT.

(Received February 2, 1871.)

The principle which I have adopted for keeping the sulphate of copper from the zinc is to allow it no access to the zinc except by true diffusion. This principle would be violated if the whole mass of the liquid contiguous to the zinc were moved toward the zinc. Such a motion actually takes place in the second form of element (that which is represented in the drawing, and which is undoubtedly the better form of the two) every time crystals of sulphate of copper are dropped into the charging-tube. As the crystals dissolve, the liquid again sinks, but not through the whole range through which it rose when the crystals were immersed. It sinks further as the sulphate of copper is electrically precipitated on the copper plate below in course of working the battery. Neglecting the volume of the metallic copper, we may say, with little error, that the whole residual rise is that corresponding to the volume of water of crystallization of the crystals which have been introduced and used. It becomes, therefore, a question whether it may not become a valuable economy to use anhydrous sulphate of copper instead of the crystals; but at present we are practically confined to the "blue vitriol" crystals of commerce; and therefore the quantity of water added at the top of the cell from time to time must be, on the whole, at least equal to the quantity of water of crystallization introduced below by the crystals. Unless a cover is added to prevent evaporation, the quantity of water added above must exceed the water of crystallization introduced below by at least enough to supply what has evaporated. There ought to be a further excess, because a downward movement of the liquid from the zinc to the level from which the siphon draws is very desirable to retard the diffusion of sulphate of copper upwards to the zinc. Lastly, this downward movement is also of great value to carry away the sulphate of zinc as it is generated in the use of the battery. The quantity of water added above ought to be regulated so as to keep the liquid in contact with the zinc a little less than half saturated with sulphate of zinc, as it seems, from the observations of various experimenters, that the resistance of water

semisaturated with sulphate of zinc is considerably less than that of a saturated solution. A still more serious inconvenience than a somewhat increased resistance has been pointed out to me by Mr. Varley as a consequence of allowing sulphate of zinc to accumulate in the battery. Sulphate of zinc crystallizes over the lip of the jar, and forms pendants like icicles outside, which act as capillary siphons, and carry off liquid. Mr. Varley tells me that this curious phenomenon is not unfrequently observed in telegraph-batteries, and sometimes goes so far as to empty a cell and throw it altogether out of action. Even without this extreme result, the crystallization of zinc about the mouth of the jar is very inconvenient and deleterious. It is of course altogether avoided by the plan I now propose.

In conclusion, then, the siphon extractor must be arranged to carry off all the water of crystallization of the sulphate of copper decomposed in the use of the cell, and enough of water besides to carry away as much sulphate of zinc as is formed in the use of the battery. Probably the most convenient mode of working the system in practice will be to use a glass capillary siphon, drawing quickly enough to carry off in a few hours as much water as is poured in each time at the top—and to place, as shown in the drawing, the discharging end of the siphon so as to limit the discharge to a level somewhat above the upper level of the zinc grating. It will no doubt be found convenient in practice to add measured amounts of sulphate of copper by the charging-tube each time, and at the same time to pour in a measured amount of water, with or without a small quantity of sulphate of zinc in solution.

As 100 parts by weight of sulphate-of-copper crystals contain, as nearly as may be, 36 parts of water, it may probably answer very well to put in, for every kilogramme of sulphate of copper, half a kilogramme of water. Experience (with the aid of specific-gravity beads) will no doubt render it very easy, by a perfectly methodical action involving very little labour, to keep the battery in good and constant action, according to the circumstances of each case.

When, as in laboratory work, or in arrangements for lecture-illustrations, there may be long intervals of time during which the battery is not used, it will be convenient to cease adding sulphate of copper when there is no immediate prospect of action being required, and to cease pouring in water when little or no colour of sulphate of copper is seen in the solution below. The battery is then in a state in which it may be left untouched for months or years. All that will be necessary to set it in action again will be to fill it up with water to replace what has evaporated in the interval, and stir the liquid in the upper part of the jar slightly, until the upper specific-gravity bead is floated to near the top by sulphate of zinc, and then to place a measured amount of sulphate of copper in the funnel at the top of the charging-tube.

Feb. 2.—General Sir Edward Sabine, K.C.B., President, in the Chair.

The following communication was read :—

“Measurements of Specific Inductive Capacity of Dielectrics, in

the Physical Laboratory of the University of Glasgow." By John C. Gibson, M.A., and Thomas Barclay, M.A.

This paper describes the instruments and processes employed in a series of experiments on the specific inductive capacity of paraffine, and the effect upon it of variations of temperature. The instruments described are the platymeter and the sliding condenser. The former of these was, in a rudimentary form, shown to the Mathematical and Physical Section of the British Association at its Glasgow Meeting in 1855, by W. Thomson. It consists of two equal and similar condensers employed for the comparison of electrostatic capacities. The sliding condenser is a condenser the capacity of which may be varied by known quantities by altering the effective area of the opposed surfaces. By means of these two instruments, along with the quadrant electrometer, the capacity of a condenser may be determined by equalizing the sliding condenser to it. The method of working, and the electrical actions upon which it depends, are described in detail. In order to determine the capacity of the sliding condenser at the lower extremity of its range, a spherical condenser, so constructed that its capacity could be accurately determined in absolute measure, was employed. An apparent discrepancy in the results obtained, arising from an inequality in the condensers forming the platymeter, is then considered, and the method of deducing the true result investigated. A series of experiments are then described which gave 1.975 as the specific inductive capacity of paraffine, that of air being taken as unity, but failed to show whether this alters with variations of temperature. An improved form of condenser, composed of concentric brass cylinders with paraffine for the dielectric, and the results obtained from it, are then described. The measurements made at different temperatures show no variation of specific inductive capacity. In order to allow to the paraffine freedom of expansion with temperature, another form of condenser was employed, and the same results obtained. A series of experiments were then made on the expansion of paraffine with temperature, in order to estimate the effect of this upon the capacity of paraffine condensers. As a mean of the results, it was found that the linear expansion of paraffine at 9° C. is .000237 per degree. Some further measurements of the cylindrical condenser were made with the same result as before. Thus all the measurements of this condenser made at temperatures ranging from -12° 15 to 24° 35 C. show no variation of specific inductive capacity of paraffine with temperature. This was found to be 1.977, that of air being taken as unity.

In a note added to the paper a description is given of an improved form of sliding condenser.

Feb. 16.—General Sir Edward Sabine, K.C.B., President, in the Chair.

The following communication was read:—

"On a Registering Spectroscope." By William Huggins, LL.D., D.C.L., F.R.S.

The short duration of the totality of the solar eclipse of December

last, led me to seek some method by which the positions of lines observed in the spectrum of the corona might be instantly registered without removing the eye from the instrument, so as to avoid the loss of time and fatigue to the eye of reading a micrometer-head, or the distraction of the attention and other inconveniences of an illuminated scale.

After consultation with the optician Mr. Grubb, it seemed that this object could be satisfactorily accomplished by fixing in the eyepiece of the spectroscope a pointer which could be moved along the spectrum by a quick-motion screw, together with some arrangement by which the position of this pointer, when brought into coincidence with a line, could be instantly registered.

I was furnished by Mr. Grubb with an instrument fulfilling these conditions, and also with a similar instrument with some modifications by Mr. Ladd, in time for the observation of the eclipse.

Unfortunately, at my station at Oran, heavy clouds at the time of totality prevented their use on the corona; but they were found so convenient for the rapid registration of spectra, that it appears probable that similar instruments may be of service for other spectrum-observations.

In these instruments the small telescope of the spectroscope is fixed, and at its focus is a pointer which can be brought rapidly upon any part of the spectrum by a screw-head outside the telescope. The spectrum and pointer are viewed by a positive eyepiece which slides in front of the telescope, so that the part of the spectrum under observation can always be brought to the middle of the field of view. The arm carrying the pointer is connected by a lever with a second arm, to the end of which are attached two needles, so that these move over about two inches when the pointer is made to traverse the spectrum from the red to the violet. Under the extremity of the arm fitted with the needles is a frame containing a card, firmly held in it by two pins which pierce the card. This frame containing the card can be moved forward so as to bring in succession five different portions of the card under the points of the needles; on each of these portions of the card a spectrum can be registered.

The mode of using the instrument is obvious. By means of the screw-head at the side of the telescope, the pointer can be brought into coincidence with a line; a finger of the other hand is then pressed upon one of the needles at the end of the arm which traverses the card, and the position of the line is instantly recorded by a minute prick on the card. A bright line is distinguished from a dark line by pressing the finger on both needles, by which a second prick is made, immediately below the other. In all cases the position of the line is registered by the same needle, the second needle being used to denote that the line recorded is a bright one.

It was found that from ten to twelve Fraunhofer lines could be registered in about 15 seconds, and that, when the same lines were recorded five times in succession on the same card, no sensible difference of position could be detected between the pricks registering the same line in the several spectra.

It is obvious that, by registering the spectra of different substances on the card, a ready method is obtained of comparing the relative positions of the lines of their spectra.

Each spectroscope was furnished with a compound prism, which was made by Mr. Grubb, and gave a dispersion equal to about two prisms of dense glass with a refracting angle of 60° .

Postscript.—I have just learned that in a spectroscope contrived by Professor Winlock for observing the eclipse of December 22, 1870, the positions of the observing-telescope are registered by marks made upon a plate of silvered copper.—February 3, 1871.

GEOLOGICAL SOCIETY.

[Continued from p. 481.]

Dec. 21, 1870.—Joseph Prestwich, Esq., F.R.S., President, in the Chair.

The following communications were read:—

1. "On Lower Tertiary Deposits recently exposed at Portsmouth." By C. J. A. Meyer, Esq., F.G.S.

The author described some exposures of Lower Tertiary deposits made during excavations for the "Dockyard Extension Works" in Portsmouth Harbour. The thickness exposed, exclusive of alluvial deposits, amounted in all to 127 feet. The beds dip S.S.W., or nearly south, $2\frac{1}{2}$ to 3 degrees. The author grouped them under the four following divisions, in ascending order:—

- | | |
|--|----------|
| 1. Clays and sands with pyrites..... | 36 feet. |
| 2. Argillaceous sands with <i>Dentalium</i> | 25 " |
| 3. Sands with <i>Lingula</i> | 8 " |
| 4. Clays with <i>Cyprina</i> and sandy clays.... | 55 " |

The author indicated the fossils contained in each of these divisions, remarking upon the range of some of the species, and upon the apparent mixture of London-Clay forms with others usually regarded as characteristic of higher or lower beds, which occurs especially in the "*Lingula*-sands." He suggested that, as the species found here present some slight differences from those occurring in other deposits, the difficulty might be got over on Darwinian principles. The author considered that the fossils did not furnish any satisfactory evidence of the true position of these beds; but, from stratigraphical evidence, he regarded them as being included in group 3 and part of group 4 of Mr. Prestwich's section of the Whitecliff strata in the Isle of Wight. He concluded with some remarks on the superficial deposits consisting of gravel and old and recent mud overlying the Tertiary beds in the section described by him.

2. "Note on some new Crustaceans from the Lower Eocene of Portsmouth, collected by C. J. A. Meyer, Esq., F.G.S." By H. Woodward, Esq., F.G.S.

Mr. Woodward, drew attention to the occurrence in the fossil state of pelagic forms of Crustacea armed with long spines on the latero-anterior angles of the carapace.

Two Eocene forms had been described by Dr. Alphonse Milne-Edwards, namely *Enoplonotus armatus* and *Psammocarcinus Hericartii*.

Two new forms, differing generically from the above, but probably referable to the same family (the *Portunidae*), were described, under the name of *Rhachisoma* (g. n.), *R. echinata* and *R. bispinosa*.

A third form, belonging to the *Corystidae*, was then noticed. This family, represented in the fossil state by the genus *Palæocorystes*, is well known in the Gault and Upper Greensand of Folkestone and Cambridge, one species ranging up as high as the Maestricht beds. The occurrence of *Palæocorystes* in the Lower Eocene is of great interest. Mr. Woodward named this new *Palæocorystes* *P. glabra*.

3. "On the Chalk of the Cliffs from Seaford to Eastbourne, Sussex." By W. Whitaker, Esq., B.A., F.G.S.

The author compared the chalk of the Sussex coast with that of the Kentish coast, and stated that it consisted of the following divisions in descending order:—

1. Chalk with flints, of great thickness.
2. Chalk with flints and nodular layers, weathering rough.
3. Chalk without flints, but with nodular layers, weathering rough.
4. Thick-bedded massive chalk without flints.
5. More thinly bedded chalk without flints, but with marly beds.
6. Chalk-marl, 50 or 60 feet.

The highest of these divisions stretches as far eastwards as Beachy Head, and forms the whole of the cliffs to within a short distance of that point.

4. "On the Chalk of the southern part of Dorset and Devon." By W. Whitaker, Esq., B.A., F.G.S.

The divisions of the Chalk were traced by the author westward from cliffs on the north side of Swa-age Bay to beyond Beer Head in Devonshire. First the succession of the beds was shown to be as in the Isle of Wight, namely:—

- | | | |
|---------------------------------------|--|--------------------------|
| 1. Chalk with flints, very thick. | | 4. Chalk without flints. |
| 2. Chalk with few flints. | | 5. Chalk-marl. |
| 3. Chalk-rock, very thinly developed. | | |

It was shown that the lower beds became thinner westward, until, at one part of the Beer-Head section, the chalk with flints rested at once on the Upper Greensand; and the following general conclusions were drawn:—

That the chalk-marl thins westward, and its bottom part becomes marked by the presence of quartz-grains, showing perhaps signs of a less deep-sea character than usual.

That the chalk without flints thins westward (from about 200 feet in the Isle of Wight) until, in Devonshire, it is but 30 feet thick, or even less.

The consequent nearness of the chalk with flints to the Greensand helps to explain the deposits of flints on some of the Devonshire hills.

Jan. 11.—Joseph Prestwich, Esq. F.R.S., President, in the Chair.

The following communications were read:—

1. "On the older Metamorphic Rocks and Granite of Banffshire." By T. F. Jamieson, Esq., F.G.S.

The author indicated three divisions in the metamorphic strata of

Banffshire:—At bottom a great thickness of arenaceous beds, more or less altered into quartz-rock, gneiss, and mica-schist; next a series of fine-grained clay-slates, in the midst of which is a bed of limestone; and then again an upper group of arenaceous strata. The author stated that the arrangement of the rocks is very similar to that occurring in Bute and Argyllshire. He remarked that the general texture of the beds is fine-grained, and considered that they were probably deposited in the depths of the sea, off the mouth of a great river, the deposition of the argillaceous strata having taken place during a period of increased depression. The deposition of the beds was said to have probably taken place after the formation of the (Cambrian) Red Sandstone and Conglomerate of the North-west Highlands, or in Lower Silurian times, the river by which the sediment was brought down being supposed to have drained the great Laurentian region to the north-west. After their accumulation the author supposed that “a glow of heat from beneath” approached them, causing expansion and the wrinkling of the mass into folds running from S.W. to N.E. The granites were considered by the author to owe their origin to the fusion and recrystallization of the arenaceous beds.

2. “On the connexion of Volcanic action with changes of Level.”
By Joseph John Murphy, Esq., F.G.S.

The author commenced by discussing the chemical theory of volcanic action, which he considered he had disproved. He remarked on the coincidence of volcanic action with elevation of the surface, but stated his opinion that the elevation of one part of the earth's surface, and the depression of another are the results of a movement of subsidence in the following manner:—The interior of the earth is constantly cooling, and as it cools it must contract. But the cold strata of the surface cannot contract in the same proportion; and as they must remain in contact with the core, they are compelled to form folds and ridges. The breaking out of volcanoes is due to the breaking of part of the earth's crust by these foldings. According to the author, “volcanic action is not the cause, but the effect of secular changes of level; and secular changes of level are due to the subsidence of the surface on the interior, as the interior contracts in cooling.”

3. “On some points in the Geology of the neighbourhood of Malaga.” By Don M. de Orueba.

After referring to the writings of previous authors upon the geology of the south of Spain, the author noticed a mountain-chain near Antequera, one branch of which, known as the “Torcal,” he described as presenting a very singular appearance from the huge blocks of stone of which it is composed. The division of the rock into separate blocks, often of the most fantastic shapes, was attributed by the author to denudation by water. The “Torcal” consists of a compact limestone, generally of a red colour, resting con-

formably on the east upon a fine-grained white oolitic marble of considerable thickness. At the divisional line between the two formations many Ammonites were said to occur; and three of these were doubtfully identified with *A. giganteus*, *biplex*, and *annulatus*. These species would indicate the deposit to be probably of Portlandian age.

The plain of Antequera was considered by the author to consist of Tertiary formations. One of these, at the south of the city, he regarded as analogous to the "Calcaire grossier." He mentioned indications of the presence in the vicinity of a Miliolitic marble, and of a limestone containing Nummulites. Between Antequera and the Torcal, he noticed a small calcareous deposit containing many forms of *Gryphæa*. The paper was illustrated by photographs of two scenes on the Torcal, and of several species of Ammonites.

LXX. Intelligence and Miscellaneous Articles.

ON THE SENSITIVENESS TO LIGHT OF THE SILVER HALOID COMPOUNDS, AND THE CONNEXION OF THE OPTICAL AND THE CHEMICAL ABSORPTION OF LIGHT. BY C. SCHULTZ-SELLACK.

THE haloid compounds of silver, which share with so many other substances the property of being altered by light, are remarkable from the fact that they are photographically excited by light; that is to say, they have the property of condensing mercurial vapour from the air (Daguerreotype), or silver when being deposited from a liquid (photography). The photographic excitation is due to the chemical change; but as it occurs before the products of decomposition of the silver-salts can be shown by other means, the photographic process is especially fitted for investigating the chemical action of the various spectral colours on the haloid compounds of silver*. All luminous rays which act chemically upon a substance are absorbed, as was shown by Draper; since chemical and optical rays of the same refrangibility cannot be separated, or rather are identical, they, of course, disappear as regards the eye. Hence by the photographic process we obtain absorption of light joined with chemical action, *chemical absorption of light*. Chloride, bromide, and iodide of silver, as I have previously shown†, may by melting be obtained in perfectly clear masses; in this condition they are only very slowly changed by light, and are especially suited for investigating *optical absorption*.

I have found that optical and chemical absorption of light exactly coincide. *All colours which in a thickness of some millimetres exert an appreciably optical absorption, effect chemical decomposition; absorption of light in these substances is always connected with chemical action.*

Iodide, bromide, and chloride of silver are sensitive to the ultra-

* In what follows, the usual collodion process was used: collodion mixed with dissolved haloid salts was poured upon a glass plate, the plate dipped in solution of silver, and after exposure developed by means of ferrous sulphate.

† Pogg. *Ann.* vol. cxxxix. p. 182.

violet of the solar spectrum; they are variously so for the visible part of the spectrum.

Chloride-of-silver collodion is only sensitive for the extreme violet, as far as about halfway between Fraunhofer's lines H and G.

Iodide-of-silver collodion is sensitive to beyond the line G.

Bromide-of-silver collodion is sensitive nearly to the line F.

Mixed iodide and bromide-of-silver collodion is sensitive to the line E, as well as *mixed iodide and chloride of silver*. The very remarkably greater delicacy of mixtures of bromide and iodide of silver has long been used in practical photography.

The optical absorption of transparent plates of these substances is seen, on being examined spectrally, to be accurately restricted to the limits given of chemical action. This is specially the case with mixtures of iodide and bromide of silver. Chloride of silver is colourless, iodide of silver bright yellow and transparent, bromide of silver of a somewhat deeper yellow; but the mixture of the two obtained by melting is orange-yellow. This difference in colour, which indicates the different delicacy of light, can be recognized by the precipitates: pure iodide of silver and pure bromide of silver are pale yellow; the precipitate with the mixed iodides and bromides, and with the iodides and chlorides is *intense yellow*.

I have formerly shown that the ultra-red heat-rays, which do not act photographically, are not at all absorbed by the haloid silver-salts; so that, including the heat-rays, the principle holds:—*The haloid compounds of silver are chemically changed by all rays which they absorb in appreciable strength.*

This connexion of absorption of light and chemical action probably holds for all substances sensitive to light. Moreover Herschel (from experiments on bleaching vegetable colours) and Draper (from experiments on the decomposition of ferric citrate) have called attention to the reciprocity of chemically active and transmitted light.—*Berichte der Berliner Chemischen Gesellschaft*, No. 4, 1871.

CHEMICAL AND MECHANICAL CHANGES IN HALOID SILVER-SALTS BY LIGHT. BY C. SCHULTZ-SELLACK.

Chloride and bromide of silver are decomposed in light, chlorine and bromine being liberated in such quantities that they are recognizable by the odour and by chemical reagents; the dark-coloured product of decomposition is therefore probably a subchloride and subbromide. If a greater excess of chlorine- or bromine-vapour be admitted, the colour disappears on continuing the illumination. In the action of light upon iodide of silver a dark coloration also sets in, although no free iodine could be shown; by a slight addition of free iodine the coloration is prevented.

As these decompositions depend upon the tension of the vapour of free chlorine, bromine, and iodine, they can be regarded as *phenomena of dissociation effected by light*. The tension of dissociation of iodine in the case of iodide of silver is then so small that iodine cannot be directly detected in the vicinity. As the tensions of dissociation in-

crease in general with the temperature, this may also be expected in the case of iodide of silver; and if at a higher temperature any liberated iodine be removed and collected, the reaction for iodine may possibly be obtained. Moreover at a higher temperature the far darker coloration, and therefore the more powerful optical absorption of the silver haloid salts, renders probable, as I have already remarked, a stronger chemical absorption and decomposition.

By my previous experiments I have not been able with certainty to detect iodine in the current of air which has been passed over heated and insulated iodide of silver; a decisive result for these experiments may be expected with the use of the summer sun.

Chloride of silver and bromide of silver may in the dark be obtained in clear lustrous crystals from their solution in ammonia, and iodide of silver from its solution in hydriodic acid. If these crystals be exposed in the presence of free chlorine (or bromine or iodine) in glass tubes to the action of light, no chemical change takes place, though a mechanical change is observable. The crystals of iodide of silver fall to powder; crystals of bromide of silver and of chloride of silver become opaque and lose their lustre. The deportment of the clear transparent layer of iodide of silver, such as is obtained by iodizing a silver mirror, is similar. The freshly prepared clear layer changes in a few minutes in sunlight to a yellowish-grey rough mass, which exhibits different colours in transmitted light, varying with the duration of the action: it first of all appears yellowish brown, then dark brown and very turbid, then red and green and blue, becoming considerably more transparent, and is finally of a pale bluish white. The ultimate colour may, moreover, be different, according to the thickness and colour of the layer and the intensity of light. These colours arise from diffraction of light, and depend upon the degree of pulverization of the iodide produced by the action of light. If the air in the interstices of the powder be replaced by another medium, the layer impregnated with lac, the colours alter and mostly lose intensity. It is probable that in what are called "coloured photographs" the colours also result from a change of structure.

Most sensitive to mechanical change is iodide of silver when it has first been formed in iodine-vapour; by lengthened keeping in the air, or by immersion in dilute silver solutions or other substances which absorb iodine, the sensitiveness is almost entirely removed, but is restored by again fuming with iodine-vapours. The mechanical change of iodide of silver is effected only by those colours of the spectrum which photographically excite iodide of silver; light which has passed through a layer of iodide of silver (and has thus been desensitized) is therefore quite inactive.

This deportment of iodide of silver may be used for preparing photographic pictures; and by the use of a photographic negative a brown picture which is *positive* by transmitted light is obtained. If light be allowed to act further, the brown colour changes into a clear blue, the shadows become bright, the *positive changes into a negative*. By hyposulphite of soda both the coherent and the pulverulent iodide of silver are obtained; by washing with dilute solution of nitrate of

silver, or by coating with a transparent resinous lac, these pictures may be preserved in the light. The coating of lac probably acts by enclosing the parts of the iodide of silver so firmly that they cannot separate.

These pictures, which I shall name mechanical iodide-of-silver pictures, are formed upon iodide-of-silver collodion fumed with iodine, which already contains iodide of silver as a precipitated fine powder, appearing transparent and of a brownish-yellow colour. The layer of powder, however, is still further disintegrated by light; and by adequate exposure under a photographic negative, there is first obtained a *positive*, which appears dark brown in transmitted light and, by a series of beautiful shadings, ultimately changes into a negative. If the layer of iodized collodion be almost allowed to dry before immersing in the silver solution, the iodide of silver will be precipitated in such fine particles that it will show these colours directly without any action of light.

It remains to be decided whether this mechanical change of the haloid silver-salts effected by light plays a part in the ordinary photographic process. By adequate exposure a visible picture may be directly obtained on the sensitive Daguerreotype plate, or on a chloride-, bromide-, or iodide-of-silver collodion plate which has been immersed in nitrate-of-silver solution: this picture is not soluble in hyposulphite of soda, and can therefore be fixed by it. The substance of this picture, which is poorer in iodine than iodide of silver*, possesses the photographic attraction for particles of mercury or silver. This *chemical* image is changed by excess of iodine into iodide of silver soluble in hyposulphite of soda; its colour changes, and it thereby simultaneously loses its *photographic property*; while the above-described mechanical iodide-of-silver picture is just formed in the presence of excess of iodine, is not again destroyed by it, and of itself has not the photographic property.

It thus appears to me proved that the mechanical change of the haloid silver-salts in the *photographically sensitive condition* is indeed quite parallel to the chemical change, but is extremely small, and that the *photographic process is inseparably bound up with chemical decomposition*.

It is worth of remark that the mechanical change of the silver haloid salts (the removal of the cohesion of the molecules) is most powerful when the chemical change (the separation of the atoms in the molecules) is least. Possibly a disintegration by light, such as has hitherto only been known in the case of realgar, takes place also in other substances which are sensitive to light; apparently crystallized iodide of tin undergoes this change, and probably also bichromate of potash.—*Berliner Chemische Berichte*, No. 6, 1871.

* This is most simply seen from the following experiment:—If a reguline surface of silver produced upon glass be superficially iodized, so that a thin layer of silver remains unchanged, by exposure a picture may be produced upon the plate and fixed by hyposulphite of soda. In the places in which light has acted, the layer of silver which gives the background of the picture has disappeared, the iodide of silver which has been exposed must therefore have given up iodine to silver. Moser, who first observed these pictures, thought they consisted of physically modified iodide of silver.

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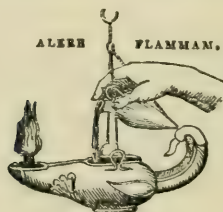
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